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January 1999

How Large an Effect Do Child Care Costs Have on Single Mothers' Labor Supply?

Evidence Using Access to Free Public Schooling

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November 17, 1998

Abstract

Public schooling for five-year-olds generates variation in child care costs. This variation allows measurement of the impact of providing free child care directly, while school entry rules naturally suggest five-year-old's quarter of birth as instrumental variables. This approach avoids both making functional form assumptions to control for selectivity bias and inducing other simultaneity problems caused by regressing quantity on price. IV estimates imply that among female heads whose youngest child is five, public schooling for the five-year-old significantly increases labor supply while reducing public assistance receipt. Female heads with a five-year-old and a younger child do not respond to five-year-old's public schooling, though married women with a five-year-old respond significantly, regardless of their youngest child's age. (*JEL* H53, I30, J13, J22)

[0] I would like to thank Josh Angrist, Susan Averett, Marianne Bitler, Deon Filmer, Bill Evans, Jon Gruber, Jeff Kling, Naci Mocan, Sendhil Mullainathan, Lant Pritchett, Robert Reville, Aaron Yelowitz, Madeline Zavodny, and participants at MIT, RAND, the University of Colorado at Denver, the University of Maryland at College Park, the World Bank, the Federal Reserve Board of Governors, and the Urban Institute for their help and useful comments. I owe an especially great debt to Jim Poterba, without whose help and moral support this paper might never have been completed. Support from an NSF graduate research fellowship is also gratefully acknowledged. This paper is a revised version of Chapter 1 of my MIT Ph.D. dissertation, "Three Essays in Labor and Public Economics".

> From now on, our nation's answer to this great social challenge will no longer be a never-ending cycle of welfare, it will be the dignity, the power and the ethic of work. . . . [The welfare reform bill] is good because without the assurance of child care it's all but impossible for a mother with young children to go to work.
>
> *President Bill Clinton*
>
> August 22, 1996

1 Introduction

Child care has become a major focal point in ongoing welfare reform efforts.[1] The economic case for child care's role in determining labor supply is simple. For parents of children too young to care for themselves, child care costs can act like a tax on wages: parents must find someone to watch their children while they work. As policymakers have sought to reduce the welfare rolls by increasing labor supply among single mothers, they have begun to focus on this role of child care costs. The 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) included both a consolidation of the patchwork system of Federal child care expenditures into a single block grant and an increase in funding for child care subsidies to low-income families, even while direct cash and other in-kind benefits were being restricted.

Policymakers seem to be betting that increased child care subsidies, perhaps together with other services (e.g. job training and job search assistance) for welfare participants, will mitigate the effects of binding work requirements and time limits on welfare participants. The question naturally arises, then, just how much bang can the child care buck deliver in moving welfare mothers into the workforce?

In this paper, I use 1980 Census data to estimate the effect of access to free public schooling for a woman's children on her labor supply and her probability of receiving public assistance. Among single women whose youngest child is five, I find that free public schooling for the five-year-old significantly increased 1979 labor supply and income from wages and salaries, while significantly reducing the likelihood that the woman received public assistance in 1979. Similar results hold for current employment and hours of work for the week before sample members completed the Census questionnaire.[2] Among female heads who have a five-year-old but whose youngest child is younger than five, there is no evidence that free public schooling for the five-year-old affects labor market outcomes. These findings provide support for the view that subsidizing

[1] The announcement in early January of 1998 that the Clinton Administration would seek nearly $22 billion in added funding underscored child care's status as a major national policy issue. According to President Clinton, the initiative's increased block grants to states are meant to "double the number of children receiving child care subsidies to more than 2 million." (The White House, Jan. 7, 1998). As I write this paper, passage of the Administration's bill seems unlikely, but there is little question that child care remains on the national agenda.

[2] This date is generally supposed to be April 1 of the Census year, though there is variation in the date when respondents receive and complete the form.

child care costs would have a sizable impact on labor supply among single mothers, provided that subsidies are available for the youngest child. Among married mothers who have a five-year-old, access to free public schooling for the five-year-old has significant effects on labor market outcomes regardless of the youngest child's age. Point estimates of these effects generally are smaller than for single mothers whose youngest child is five, but it is not possible to reject the null hypothesis of identical responses.

The paper makes two significant contributions to the child care literature in general, and the literature on child care costs and single mothers in particular. First, I am able to answer an important policy question to which previous analyses have been much less suited: how large an impact can be expected from direct provision of child care? While authors of previous studies have been interested in this question, none has addressed it directly. Authors often have been interested in the effects of providing free child care, i.e. a 100% subsidy for child care expenditures. They address this question by estimating models of the sort described in the previous section, after which they predict labor supply with estimated child care expenditures set to zero.[3] Since no previous paper has used a source of freely provided child care, this approach typically involves predicting far out of sample with respect to child care costs.

Second, my use of access to public schooling as a source of variation in child care costs, and quarter of five-year-old's birth to construct instruments for public school enrollment, relies on a plausible natural experiment. This approach allows me to avoid potential selectivity bias arising from joint determination of child care and labor supply decisions without relying on statistical assumptions regarding the distribution of unobservable factors. Moreover, I am able to avoid bias due to any simultaneous relationship between labor supply and child care prices (for example, because labor supply requires purchase of child care, and the equilibrium quantity and price of child care are jointly determined). I will argue below that results from previous child care studies are highly sensitive to model specification, in a way that suggests lack of identification is a glaring problem in these studies.

Descriptive statistics certainly point to the importance of child care costs. One approach – the one that underlies my identification strategy – is simply to compare labor force participation rates among single mothers whose youngest child is aged between six and eighteen with participation rates among those whose youngest child is younger than six. The usefulness of this indicator may seem obvious: mothers of only older children need not pay for child care during school hours, while mothers of preschoolers must.

Among divorced women with children younger than 18, 85% of those whose youngest child was six years of age or older participated in the labor force in 1995. By contrast, only 73% of divorced women whose youngest child was under six years old participated.[4] The (absolute) difference was similar among never-

[3] Connelly (1990), Blau and Robins (1988), and Michalopolous et al. (1992) are examples.

[4] These and other descriptive statistics in this section were drawn from U.S. House Committee on Ways and Means (1994, 1996). Institutional details regarding Federal child care programs may also be found in those sources.

married women: 67% of never-married women with children aged between 6 and 18 (but none aged 5 or younger) participated, compared to only 53% of those with children aged younger than five.[5] Of course, child care costs are unlikely to be the only difference between single mothers with and without preschool-aged children, but differences like these are eye-opening.

Data from the Survey of Income and Program Participation (SIPP) for Fall 1993 suggest that observed averages in child care expenditures among families who purchase child care are consistent with these large differences in participation rates. Among single-parent families who paid for child care for their preschoolers, average expenditures on child care were about $60 per week.[6] Thus monthly expenditures would have been about $250; for a woman working 40 hours a week, this figure would translate to an hourly rate of $1.56. At the 1993 Federal minimum wage of $4.25, a per-hour cost of this magnitude translates to a wage tax of 37%, which is large, to say the least. Little wonder that child care costs should be a focus of concern in the welfare debate.[7] The remainder of this paper proceeds as follows. In the next section, I discuss existing literature on child care costs and women's labor supply. In section 3, I discuss the role of access to free public schooling in affecting the child care costs that women face. I address potential endogeneity of school enrollment by explaining how one may use information about the five-year-old's quarter of birth to construct instruments for public school enrollment. I discuss my dataset in section 4. I present the main estimates in section 5. In section 6, I undertake a very rough cost-benefit analysis of the effects of providing child care through the public kindergarten system. My results suggest that for women whose youngest child is five, a dollar spent on kindergarten leads to an increase in labor income of 93 cents (some of which would be returned to various levels of government through greater tax revenues and lower welfare expenditures) government. I find no apparent gain in labor income among either married women with a five-year-old or single women with a five-year-old but whose youngest child is younger than five. I conclude in section 7.

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[5] In fact, among never-married women, the differential has often topped twenty points during the last decade. The biggest gap occurred in 1993, when 70% of never-married women with only school-aged children participated in the labor force, by comparison to just 47% of women with preschoolers; the difference of 23 percentage points thus equals approximately half the LFP of women with preschool-aged children.

[6] Even among families (married or not) with incomes below the poverty line, average expenditures were still $50.

[7] Note that the assumption of 40 hours is likely to be sanguine; many women in the SIPP sample likely worked many fewer hours than that, raising the implied hourly rate of child care costs. On the other hand, many low-wage women may have access to family members and neighbors who are able to care for their children during at least some portion of the work day. The figures discussed in this paragraph of the text are meant only to paint a broad picture of the potential impact of child care costs on low-wage women's budget sets.

2 Existing literature

The literature on women's labor supply and child care costs is reasonably large, though it has focused mainly on married women. Researchers have used several sources of variation. Most common is observed household child care expenditures; Connelly (1989, 1990, 1992), Berger and Black (1992), Averett et al. (1997), and Michalopoulos et al. (1992) are examples. Blau and Robins (1988) use site-average expenditures among participants in the 1980 Employment Opportunities Pilot Project as an index of costs facing married women. Barrow (1996) uses state averages of child care workers' wages and also applies regression techniques to household expenditure data to create two cross-state indexes of relative costs. Ribar (1992) uses a combination of data on household expenditures, area averages in wages, and characteristics of state child care regulations. Others have used variation in tax rates: Michalopoulos et al. (1992) and Averett et al. (1997) use static variation across states or points in the income distribution, while Meara (1996) uses variation in state tax credits over time.

Nearly all of these papers have relied on nonlinear models for identification. Many of them use some method of selection correction (typically based on assumptions regarding the joint distribution of unobservables across the selection and main equations) to account for endogeneity of child care expenditures and labor force participation;[8] Connelly (1989, 1990, 1992), Ribar (1992) and Michalopoulos et al. (1992) are typical examples.

Estimated child care cost elasticities of labor supply among married women range from as low as 0 to as high as 0.9.[9] For single mothers, authors have found small point elasticities – as low as 0[10] – as well as larger effects. Using data from the 1987 and 1988 SIPP panels, Kimmel (1995) estimates a labor force participation elasticity of 0.35 among all poor single mothers; among blacks, the elasticity was 1.36, while for whites it was 0.35.[11] General Accounting Office (1994) finds that the employment rate of poor mothers would increase from 29% to 44% if child care costs were fully subsidized, implying an (arc) elasticity of 0.52.[12] Berger and

[8] Blau and Robins (1988) do not correct for selection, relying only on area averages in expenditures. Meara's (1996) use of differences-in-differences methodology to estimate responses to changes in laws governing state tax credits plausibly avoids the need for such procedures.

[9] Unless explicitly noted, references to child care cost elasticities of labor supply are stated in magnitudes, with the understanding that the elasticities are negative in sign.

[10] This estimate comes from Michalopoulos et al. (1992), who compute their elasticity conditional on positive hours of labor supply and positive expenditures on child care. Thus their estimated elasticity does not include effects of child care costs on the participation decision, so it is likely to be an underestimate.

[11] One might worry about conditioning on poverty status, since doing so raises possible endogeneity and selection problems.

[12] This study pools single and married mothers.

Black (1992) find that the average subsidy from a Kentucky program increased employment by an upper bound of 25.3% (not percentage points). However, Berger and Black do not report average child care costs in the sample, so no elasticity may be computed. Connelly (1990) finds that a 50% subsidy of child care costs would reduce AFDC participation from her sample average of 20% to 11%, for an (arc) elasticity of 0.9. However, she finds that subsidies would have little impact on labor force participation among women in her sample, a finding which is difficult to reconcile with such a large welfare participation elasticity.

This wide variation in the estimated impact of child care costs on labor supply has been described as one of the most important questions facing child care researchers.[13] Kimmel (1996) presents a very interesting attempt to reconcile the disagreement among different studies. Using more recent data than most previous papers,[14] Kimmel (1996) first estimates her preferred specification. Her equation of interest relates labor force participation to child care costs, the mother's wage, and other controls.[15] Kimmel attempts to control for potential sample selection bias arising from the fact that only some mothers are observed purchasing child care, and only some mothers are observed working. She uses a bivariate (reduced form) probit model to predict labor force participation and purchase of child care services jointly. The estimated coefficients from these equations are used to correct for sample selection in equations predicting wages and child care costs, and these predicted values are then included in the final probit equation predicting labor force participation. She then compares her results to those generated using her data and Ribar's (1992) and Connelly's (1992) model specifications.[16]

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[13] See Connelly (1992) and Kimmel (1996).

[14] Kimmel (1996) uses 1987 SIPP data, while many other papers in the child care literature (including those Kimmel focuses on) use 1984 SIPP data.

[15] The controls are mother's age, age squared, labor income, years of completed education, and total number of own children present in the family, whether the mother has any children aged 0-2, 3-5, or 13-17, whether the mother is nonwhite, whether she lives in the south, whether she lives in a metropolitan area, whether she is "unhealthy", whether any of her children are "unhealthy", her state's average observed (i.e. not the statutory maximum) AFDC grant for a family of 3, her state's Medicaid expenditure per recipient, and the number of other adults living in her household.

[16] Kimmel's preferred specification differs from Ribar's (1992) in two ways. First, Ribar uses a different definition (described below) of child care costs. Second, in estimating his price of care equation, Ribar includes only a single sample selection correction term, rather than the bivariate one used by both Kimmel (1996) and Connelly (1992). Kimmel also includes in her structural probit equation several variables that Ribar does not: whether the mother has any sick children, whether the mother is "unhealthy", her state's average AFDC grant for a family of 3, and her state's Medicaid expenditure per recipient. The only substantive difference between Kimmel's (1992) preferred specification and Connelly's is that Connelly does not include several variables in her structural probit equation for employment (variables included in Kimmel's but not Connelly's specification are: the number of other adults living in the mother's household, whether she has any children aged 13-17, and the total number of own children in her family.)

Several of Kimmel's findings merit description here. First, her structural estimates suggest labor force participation elasticities with respect to child care cost of 0.92 for married mothers and 0.22 for single mothers.[17] Second, Kimmel is able to explain some of the difference in married mothers' elasticities estimated by Ribar (1992) and Connelly (1992) by using her own data and those authors' specifications. Ribar estimates an elasticity of 0.74, while Connelly's estimate is 0.2. Ribar (1992) has commented that part of the difference between his and other estimates may result from differing definitions of child care costs: while he defines price of care as expenditures per hour of care, other authors (including both Connelly (1992) and Kimmel's (1996) preferred specification) define price as expenditures per hour of mother's work. Using Ribar's model specification and variable definitions, Kimmel (1996) estimates an elasticity of 0.89, while her replication of Connelly's (1992) specification and variable definitions yields an elasticity of 0.42.

Kimmel argues that these results suggest that "much of the variation in child care price elasticity estimates across papers in the literature is likely due to equation specification." However, her replication point elasticities exceed Ribar's and Connelly's, respectively, by 20% and 110%. Even if these differences are largely explained by wide confidence intervals (which is not clear, since no standard errors are presented for the elasticities themselves), that would simply mean that estimates in the literature are too imprecise for making reliable policy judgments. Hence Kimmel's conclusion that "researchers in this field must be very careful to explain their selection of variables to include in the various equations, and conduct sensitivity analysis to determine the importance of the specification chosen" seems especially appropriate.

Kimmel (1996) repeats for single mothers the replications and sensitivity analysis just described. She writes that "The single mother estimates are much more sensitive to specification and model changes." In fact, while Kimmel's preferred specification yielded an elasticity of 0.2 computed from a coefficient not significantly different from 0, replications of Connelly (1992) and Ribar (1992) yielded coefficients significantly different (at the 5% level) from 0 and generated the following elasticities:

(*i*) -0.54 (Connelly)

(*ii*) +1.38 (Ribar's specification, but with child care costs defined as expenditures per hour worked)

(*iii*) -4.54 (Ribar's specification and variable definitions, i.e. with child care costs defined as expenditures per hour of care purchased)

The sign on the second elasticity above is not a typographical error–taken at face value, Ribar's specification with Kimmel's (1996) variable definitions suggests that an increase in child care costs of ten percent should *raise* labor force participation among single mothers by nearly fourteen percent. At the same time, Ribar's specification with his variable definitions suggests that a ten percent increase in child care costs

[17] The estimated coefficient on the predicted child care costs variable is statistically significant at the 0.01 level for Kimmel's married women sample, while for single mothers the coefficient is statistically indistinguishable from 0 (its *t*-statistic is -0.49).

should *reduce* labor force participation among these women by forty-five percent.

One potential explanation for this wide model sensitivity is the reliance on methods identified through cross-equation normality assumptions on unobserved variables. Using simulation methods, Goldberger (1983) and Arabmazar and Schmidt (1982) have shown that the Tobit model is highly sensitive to misspecification of the disturbances, even when the true distribution is similar to the standard normal. Inasmuch as the Tobit model and the models typically used in the child care literature each rely on maximum likelihood estimators that are inconsistent when normality is violated, such results suggest the possibility that misspecification of the residual distribution may be a major cause of differences in estimated effects. On the other hand, a replication of Mroz's (1987) sensitivity analysis of models of married women's hours of work, by Newey et. al. (1990), suggested little difference between estimates generated via two-step normal methods and those computed via two more robust semiparametric selection models.[18]

However, the concern in the literature about sample selection seems to have obscured another problem that is likely to be quite serious. The typical approach assumes that in the absence of sample selection, regressing labor supply on observed expenditures would raise no econometric problems. However, this assumption is unlikely to be valid, for the same reasons that one cannot simply regress observed quantity of a good on its observed price — one doesn't know whether the parameters of a supply equation, a demand equation, or some combination of the two has been estimated.

To see the analogy, suppose that one is estimating the effect of child care costs on hours of labor supply, and that sample members must purchase child care for each hour they work. Then the left hand side variable of the hours equation is identical to the quantity of child care purchased by each sample member. Suppose also that hourly prices paid for child care are observed and do not vary with the number of hours of care purchased or the number of children placed in care. Then a regression of labor supply on child care price is equivalent to a regression of child care quantity on child care price. Now imagine grouping the data by geographic market and running the grouped-means regression; the expected value (or probability limit) of the resulting coefficient estimates is equal to that for the microdata regression.[19] If we suppose also that no other people purchase child care, then we are simply regressing equilibrium (average) quantity of a good on equilibrium price, which is Haavelmo's (1943) classic example of simultaneous equations bias.

[18] This general approach would be more difficult to implement in child care studies, which typically seek to correct for selection into both the paid labor force and the use of paid child care. However, the underlying approach of comparing estimates from MLE methods to those from methods that do not rely on normality for consistency is a good one. Perhaps a useful exercise for future work in the spirit of earlier child care studies would be computation of semiparametric estimators alongside the more typical maximum likelihood ones. Such an approach would allow researchers to distinguish distributional misspecifications from other possible explanations for the sensitivity of observed results.

[19] See, for example, Greene (1993).

There is clearly nothing crucial about the simplifying assumptions made in the previous paragraph. In fact, adding consideration of quality or economies of scale (in either the number of children placed in care or the number of hours purchased for a given child) would simply make identification more difficult.[20] Thus one must ask what is moving child care costs around in previous studies without independently affecting labor supply. The endogeneity of expenditures seems to be a severe problem for researchers attempting to identify child care effects from (nonexperimental) cost data.

It is worth describing the ideal setting in which empirical estimates could be generated. In a perfect world, one would randomly assign child care prices to sample members and simply compare outcomes among those facing high prices to outcomes among those facing low prices. Absent such an experiment,[21] one needs an instrumental variables strategy. To be sure, nearly all of the papers discussed above have employed some sort of instrumental variables strategy, as they predict child care costs using specifications that included variables not included in their labor supply equations. However, it seems fair to say that the instruments typically used (e.g. region dummies and household members' or children's age structure variables) are either unlikely to be strongly related to child care costs or are unlikely to satisfy the exclusion restriction for the labor supply equation. One promising exception would seem to be using state-level rules and regulations of the child care industry (see Kimmel [1996], for example). However, such variables typically have very weak effects in the child care cost equation.

The fact is, no one has yet found a good instrument for prices. A primary advantage of using access to public schooling, then, is that there is no need to find such an instrument. Access to public schooling affects required expenditures on child care because public schooling is free. In turn, quarter of birth variables are available as instruments public school enrollment status, because QOB affects access to public schooling in ways unlikely to be correlated with labor supply outside of its effect on access to schooling. Thus estimates

[20] For example, authors typically do not have price data, but instead observe only household expenditures on child care. Moreover, hourly prices are unlikely to be constant, because of both economies to scale and variations in quality purchased. A standard approach is to use expenditures per hour of work (see the discussion of Kimmel (1996) in the text above), which may be written as $q_i p(q_i)/l_i$, where q_i, p, and l_i are, respectively, i's quantity of child care purchased, the function mapping that quantity into an average hourly price, and i's labor supply. Suppose that child care prices are nonlinear in the quantity of child care purchased. If labor supply and child care demand are not identical, nothing more can be said about expenditures per hour of work; on the other hand, if $q_i = l_i$, then expenditures per hour of work are just $p(q_i) = p(l_i)$. In either case, the RHS cost variable is a nondegenerate function of labor supply (at the individual level), causing another source of endogeneity bias.

[21] None has yet been conducted to my knowledge, though Robins and Spiegelman (1978) do use data from the Seattle-Denver Income Maintenance Experiments to estimate a child care demand equation. In their sample, people face differing Federal marginal tax rates. Since child care was a deductible expense, Robins and Spiegelman argue that they can identify parameters of their child care demand equation. Such data clearly cannot identify a labor supply equation, however, since there is no way to separate the effects on labor supply of greater marginal tax rates *per se* from the effects of associated increases in the deduction for given child care costs.

generated using quarter of birth instrumental variables work for the same basic reasons that a random assignment experiment would.

3 Model Specification and Identification

In this section, I begin in subsection 3.1 by describing the basic econometric specifications on which I will focus. In subsection 3.2 I provide a simple budget constraint-driven discussion of how estimated effects from these specifications should be interpreted economically. In subsection 3.4, I discuss the need for an instrumental variables approach to deal with likely endogeneity of public school enrollment status. Lastly, in subsection 3.5 I describe both my QOB strategy and the existing literature in which QOB variables have been used as instruments.

3.1 Specification

The models I estimate all have the following basic structure. The unit of observation is the mother, while the structural equation of interest, i.e. the second stage, is

$$y_i = \beta_0 + \beta_1 \text{enrollment}_i + \beta_2 X_i + \epsilon_i, \tag{1}$$

where y_i is the value of the outcome measure for woman i, $enrollment_i$ is a dummy variable indicating whether the woman's five-year-old is enrolled in public school, X_i is a vector of control variables, and ϵ_i represents unobservable characteristics, which may be correlated with $enrollment_i$. The coefficient of interest is β_1, which is unidentified by the OLS estimator if ϵ_i and $enrollment_i$ are correlated.

The first stage (i.e. the reduced form for the public school enrollment status of the five-year-old) is

$$\text{enrollment}_i = \delta_0 + \delta_z Z_i + \delta_x X_i + \eta_i, \tag{2}$$

where Z_i is a vector of instrumental variables, δ_z and δ_x are coefficient vectors, and η_i represents unobserved characteristics affecting enrollment decisions. The fundamental identifying assumptions are that $\delta_z \neq 0$ and $E[Z_i'\epsilon_i|X_i] = 0$. That is, given that quarter of birth variables affect enrollment status, β_1 is identified if quarter of birth is not otherwise associated with y_i.

In the estimates reported below, Z_i includes either a dummy variable for being born in the first quarter or a set of dummy variables for being born in each of quarters 2-4, depending on the model specification. The vector of controls, X_i, is either empty or includes variables specifying race, age, and education of the mother; characteristics regarding the age structure of both the mother's own children living in the household and other household members; maximum welfare benefit levels for the given family size; and fixed effects for state of residence at completion of the Census questionnaire and for the five-year-old's state of birth.

I examine seven outcome measures: weeks of work in 1979, usual weekly hours in 1979, employment in 1979, wage and salary income in 1979, whether public assistance was received in 1979, hours of work last week, and current employment status. My public school enrollment variable refers to the five-year-old's status as of the week before completion of the questionnaire.[22] Because a child would have had to enroll at the beginning of the school year (September 1979) in order to be enrolled in public school in March 1980, the enrollment variable is appropriate for analyzing 1979 as well as 1980 outcome measures.

Nonetheless, 1979 outcome measures raise an interpretation problem, since even an eligible five-year-old in 1980 generally will have had access to public schooling only for the months of September through December. Therefore, estimates using 1979 labor supply variables will be lower than what one would expect over a full year from providing free child care.

3.2 Some simple budget constraint analysis

Before describing the identification issues on which I focus for much of the rest of the paper, it will be useful to discuss what effect is actually estimated, given identification. Unlike per-hour vouchers or subsidies (whether paid to the demand or supply side of the child care market), public schooling provides free child care for a fixed number of hours during each of five days a week.

Figure 1 depicts the effects of free public schooling on a woman's budget constraint under the assumption that she has one child to whom public school is being provided (or alternatively that all children are being provided schooling simultaneously). Ignoring child care costs, the woman has a net (of Social Security and other wage taxes) wage w equal to the slope of the line $BC_{nochildcarecosts}$. When the woman takes 24 hours a day of leisure, she works 0 hours and thus in the absence of government welfare programs she has no income for consumption. The government provides her a minimum income at point G, which depicts the maximum welfare benefit. Benefits are reduced dollar for dollar between leisure hours of 24 and leisure hours of L^*_{afdc}, where the flat part of the budget constraint due to welfare's 100% tax rate meets the (no-child care costs) wage line.[23]

The budget constraint is flat between leisure of 24 and of L^*_{afdc} because of the child care disregard, which allows a capped deduction (typically for the first $175 a month, per child, spent on child care) from the

[22] Not all respondents receive or complete the questionnaire at the same time, although April 1 of the Census year is typically considered the reference day by the Census Bureau. However, both Passover and Good Friday occurred in the first week of April, 1980, which could have affected responses regarding hours of work last week. However, occurrence of these holidays seems unlikely to affect either current employment or five-year-old's enrollment.

[23] In practice, welfare rules have provided for a lag after the woman enters welfare during which she faces a benefit reduction rate (BRR) of only 67%, and there is a disregard for part of her earned income (the size of the disregard has varied over the years but was $30 when PRWORA was passed).

implicit AFDC tax. The woman must spend a dollar on child care to get one back via the disregard, so the effective tax rage the woman faces is 100% (that is, the disregard's presence effectively neutralizes child care costs). As drawn, Figure 1 assumes that total child care costs per child amount to less than the disregard.[24]

However, with no public schooling access (and no other child care subsidy) the woman pays c per hour in child care costs, so that her effective budget constraint in the absence of welfare is given by $BC_{fullchildcarecosts}$. Thus when welfare eligibility ends at leisure of L^*_{afdc}, there is a discrete drop in income as the woman now must pay inframarginal child care costs that were previously AFDC-deductible.[25]

When we add access to free child care via public schooling, we eliminate the implicit wage tax associated with child care costs, so that (if she doesn't participate in welfare) the woman's effective wage again is w.[26] Thus for as many hours as the woman has school-based child care, her budget constraint is again given by $BC_{nochildcarecosts}$. Letting school be provided for $h_{school} = 24 - L^*_{school}$ hours, then L^*_{school} becomes another cutoff value of leisure. That is, if she works more than $h_{school} = 24 - L^*_{school}$ hours, she must pay child care costs again.

Putting all of this together, we may characterize the woman's budget constraint as follows:

(i) If she works between 0 and h_{afdc} hours, her wage at the margin is 0 and her consumption is the maximum welfare benefit.

(ii) If she works $h \in [h_{afdc}, h_{school}]$ hours, her wage at the margin is w and her consumption is wh.

(iii) If she works $h \in [h_{school}, 24]$ hours, her wage at the margin is $w - c$ and her consumption is $(w - c)h + ch_{school})$.

Thus public schooling effectively provides a marginal wage subsidy over the range hours interval $[0, h_{school}]$. However, for hours greater than h_{school}, the subsidy is an inframarginal one, i.e. it operates effectively as an income transfer of ch_{school} that is tied to labor force participation.

We can now describe the effect of these budget constraint changes on labor supply choices. Figure 2 depicts a typical case in which a woman would not participate in the labor force without child care subsidies.

[24] Were child care costs to exceed the disregard, there would be a region between leisure of L^*_{afdc} and 24 hours where the budget constraint would have positive slope: the 100% BRR and child care costs together would reduce income by more than a dollar for every dollar earned.

[25] Note that the discrete fall in income occurs even if we drop the assumption that the disregard exceeds child care costs for women on AFDC. The reason is that the disregard subsidizes child care costs only so long as a woman participates in AFDC, so that when she crosses the (before-child care costs) breakeven point L^*_{afdc} and loses AFDC eligibility, she also loses the benefit of the disregard. To conserve space, I ignore a more detailed description of this case.

[26] If she does participate in welfare, her after-tax wage is 0: she earns w per hour, all of which is taxed away by AFDC.

The highest achievable indifference curve in this case is the one intersecting point G, where she works 0 hours and collects the maximum welfare benefit. When public schooling is available, however, the woman is able to do better than point G by working $h_E = 24 - L_E^*$ hours. At this level of labor supply, her indifference curve is just tangent to the budget constraint $BC_{pubschool}$ at point E. This figure should convince the reader that child care subsidies via public schooling will have an unambiguously positive effect on labor force participation and an unambiguously negative effect on welfare participation. Note that this analysis also applies for the weeks of work last year variable, since it is simply the sum of 52 weekly participation indicator variables.

Unlike the case for participation variables, the effect of access to free public schooling on usual weekly hours, last week's hours, and earned income is ambiguous. Consider the case for the usual weekly hours variable. For women whose hours would have exceeded h_{school} even in the absence of public schooling access, access simply provides a discrete income transfer. As long as leisure is a normal good, this transfer will reduce hours worked. Figure 3 depicts this case.

On the other hand, women whose hours without child care subsidies would be less than h_{school} (but greater than h_{afdc}, so that they would work without subsidies) receive a wage subsidy if they don't raise hours above h_{school}. In this case, the increase in their wage induces both income and substitution effects, as with the textbook case of an exogenous increase in the wage. So long as the income effect does not dominate (which is unlikely for low-wage women), hours will increase for these women.

Because I do not model participation decisions distinctly from hours decisions (conditional on participation), the unambigous increase in total hours that occurs because of the participation effects described above also contributes to the effect on total hours. Note that the difficulty in sorting out the direction of the effect is not due to the fact that I use linear regression techniques rather than, say, tobit methods to estimate my effects.[27] To summarize this subsection, participation variables are predicted to respond unambiguously to

[27] Linear regression techniques are appropriate when one is interested in estimating the impact of some variable x on the mean of an observed variable y even when the observed variable y takes a minimum (say, at 0). The standard concern that justifies tobit analysis is that one is actually interested in some underlying variable y^*, all of whose values are not observed. For example, y^* could be income and y could be top-coded income. In the present case, I am interested in hours of work, not in some other variable that can take either negative or postive values (e.g. "desired hours"). Hence tobit estimates do not really answer the question I am asking: such estimates would need to be scaled in order to yield the marginal effect of x on y, rather than on y^*. Nonetheless, I did run such regressions as a check, with the expected result that the appropriately scaled tobit results (computed with predicted values of public school enrollment generated via a first-stage linear regression) were effectively identical to the 2SLS results reported below.

Moreover, tobit equations restrict the coefficient (though not the marginal effect) on x to be the same for both parts of the equation (i.e. both whether y^* is positive, and its value conditional on its being positive). Hence tobit estimates cannot separate out a positive effect on participation from a negative one conditional on participation. To do that, one would need to run a participation equation followed by a linear regression of y on x for all observations having $y > 0$. This approach is of course the standard Heckman two-step procedure (maximum likelihood estimation of the same model is also possible). However, I do not

public schooling access, while hours and income variables have opposing effects. Even in the case of participation variables, however, my results cannot identify the effect of an hourly child care subsidy, since for many women in the sample, the subsidy provided by public schooling is inframarginal, shifting their budget constraints out in a parallel fashion. While the child care price elasticity of labor supply is a crucial policy variable, I am unable to shed light on it.

However, the results reported below do provide a great deal of information regarding the effects of directly provided child care. This question is itself an important one, for two reasons. First, some proposals for subsidizing child care do in fact include direct provision as either one part or their focal point. Second, if women did not respond to the subsidy provided by direct child care provision, it would seem unlikely that they would respond to hourly subsidies. That the women in my sample clearly do respond, and in the ways that economic theory predicts (at least for participation variables), is *prima faciae* evidence that child care subsidies are a promising avenue for increasing labor supply and reducing welfare reliance of single mothers.

3.3 Five-year-olds and the generalizability of the results

One potential shortcoming of the approach used here is that variation in child care costs occurs only among five-year-olds. This fact begs the question of whether the estimated treatment effects would be similar among mothers whose (treated) children are of different ages. In particular, it seems plausible that the treatment effect of free child care would differ among, say, mothers of infants and mothers of five-year-olds. There is no way for me to measure such differences with the data at hand. However, it seems plausible that for at least some children's ages, the effect should be similar to that for mothers of five-year-olds. Mothers of four-year-olds would seem like a candidate group for this argument.

It is worth noting, however, that this issue has been completely ignored in previous work on child care, which has assumed that any age specificity is captured entirely by shifts in the regression intercept. That is, authors often include variables in either or both of the child care cost and labor supply equations to control for main effects of children's age. Other things equal a typical labor supply equation is modeled as

$$y_i = \alpha + \beta_0 A_i + \beta_1 C_i + \epsilon_i$$

where A_i is the age of woman i's child and C_i is the child care cost she faces. This approach assumes that child's age affects total labor supply but does not affect the child care cost effect. Incorporating such an interactive effect would require a specification of the form

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have any instruments to predict participation: all available variables should affect both participation and hours conditional on participation. Hence such a model would be identified only on functional form, and there seems little gain from the exercise.

$$y_i = \beta_0 + \beta_1 A_i + \beta_2 C_i + \beta_3 A_i * C_i \epsilon_i,$$

so that the effects of child care costs are allowed to vary by the child's age. Without such an interaction effect, regressions like this one can estimate only average child care cost treatment effects (and of course all the econometric problems described above still apply).

Since previous literature has focused on estimating C_i in order to avoid selection bias problems (see the discussion above), a separate measure would have to be estimated for each relevant age category. A more likely approach would be to run separate models for each age group thought to have a distinct treatment effect. Given the small sample sizes typically encountered in previous studies, however, it seems unlikely that one would be able to generate statistically significant estimates of the child care cost effect.

In summary, the nature of the variation available here – access to public schooling for five-year-olds – limits the generality of my results. However, in my view, this limitation is actually less burdensome here than in previous studies, particularly in light of the questions raised in the previous section.

3.4 Potential endogeneity of public school enrollment

While access to free public schooling is a promising source of exogenous variation in child care costs, there are two reasons why one cannot simply compare labor market outcomes among single mothers with and without preschool-aged children. First, even with identical preferences and under otherwise identical circumstances, two women's labor supply and welfare participation decisions may vary directly as a function of their children's ages. To take an obvious example, labor supply decisions of a woman with a newborn and the same woman with an 18-year-old are likely to be quite different,[28] quite apart from any effects related to child care costs.

Moreover, other things equal, the population of women with children aged less than five is likely to differ in systematic ways from the population of women with only older children. For example, women with higher wages face greater opportunity costs from withdrawing from the labor force. Thus they may be less likely to have children than women with lower wages. As a result, a 30-year-old woman with a seven-year-old and a four-year-old may not be comparable to a 30-year-old woman who has only a seven-year-old, because the latter woman may have chosen not to have another child for nonrandom reasons. Labor supply and welfare participation differences between these two women therefore may be due partly to differences in child care costs, but also to differences in unobservable factors related to ability and/or offered wages. I address this

[28] Of course, it might be difficult to imagine the appropriate counterfactual comparison, since it is unlikely that a woman's preferences regarding labor supply and welfare participation remain constant over such a long time period.

problem by focusing on only those women who have a five-year-old child, so that I am identifying effects off of much finer variation. For this and other reasons described below, I split the sample into women whose youngest child is five and women who also have a child aged younger than five.

The second problem with simple comparisons between mothers whose children are enrolled in public school and those whose children are not is that, depending on their five-year-old's date of birth and state of residence, parents have discretion over their children's age at enrollment.[29] Under such circumstances, a mother's choice to enroll her child may be correlated with unobservable factors that also affect her labor supply, raising potential endogeneity concerns.

For example, one might expect a mother who wishes to work in the labor market to enroll her five-year-old precisely because doing so avoids child care costs. Mothers less interested in working will enroll their children less frequently, so that the population of women enrolling their children is simply more attached to the labor market than the population of women not enrolling their children. In this case, OLS estimates would be upwardly biased.

Another potentially more serious problem arises because some mothers will enroll their children in private school, rather than public school. Suppose for the moment that private schools have the same age-at-entry rules as public schools. Then if mothers who enroll their children in private school differ systematically from those who enroll their children in public school, a regression of labor supply on a public school enrollment dummy will yield biased estimates. For example, mothers enrolling their children in private school are likely to have higher wages than those enrolling their children in public school (simply because of the cost of private school). This effect suggests that OLS estimates will be biased downward, and will even possibly be negative (as indeed I find below).[30]

In fact, the problem is made worse because private schools need not have the same policy on age-at-entry. Worse still, "private school" may not mean the same thing to respondents as it does to analysts. For example, the Census enrollment question includes nursery school as a type of school enrollment. Since many mothers may consider daycare centers to be nursery schools, or may use nursery schools as sources of both daycare and early childhood education, I will be unable to distinguish private nursery school enrollees from children enrolled in daycare centers. Since many working single mothers enroll in daycare centers children not yet

[29] Nearly every state has a maximum age by which children must enter school; this age is generally one to two years older than the minimum allowable entry age. Parents of children who meet the minimum age for entry, but who are not yet old enough so that they are required to enroll, decide whether to enroll their children.

[30] Given identical age-at-entry rules for public and private schools, we could run an OLS regression on a dummy for any enrollment to avoid this problem. However, the resulting estimate would average over the effects of free public schooling on two populations: those mothers who choose to enroll their children in private schools even when public schools are available, and those who choose to enroll their children in public schools. Presumably it is only women in (or like those in) the second population whose labor supply is likely to respond to provision of new school-based free child care.

eligible for public school, we would expect mothers who indicate on the Census form that their children are enrolled in nursery school to have greater attachment to the labor force than those whose children are not, again raising the possibility of negative bias in OLS estimates.

3.5 Using QOB to Construct Instrumental Variables

Clearly, simple comparisons across mothers whose children are enrolled in public school and those whose children are not will fail to identify the effect of providing access to public schooling. This is the reason why the instrumental variables approach is necessary: by affecting who has access to public schooling for reasons otherwise unrelated to mothers' labor market decisions, five-year-old's quarter of birth allows identification of the treatment effect of interest.

This approach works because most states have cutoff dates by which a child must have been born in order to start school when five. Table 0 shows 1975 minimum entrance ages for kindergarten by state. As the table shows, 1 state had a first-quarter cutoff, 14 states had third-quarter cutoffs, 25 states had cutoffs in the fourth quarter, none had second-quarter cutoffs, and 11 (including the District of Columbia) had no state-wide cutoff.[31]

QOB variables were first used as instruments by Angrist and Krueger (1991) in a now well-known study examining the return to years of educational attainment. Because minimum age for dropping out of school varies by state (mostly between states that have age-16 requirements and those that have age-17 requirements), and because QOB affects when children can start school, Angrist and Krueger are able to demonstrate a relationship between educational attainment and quarter- and state-of-birth (SOB).[32] Angrist and Krueger (1991) also report specifications that use both QOB dummies and QOB×SOB dummies as instruments for educational attainment, while controlling for main SOB effects.[33]

Well known criticisms of Angrist and Krueger's (1991) use of QOB and SOB variables have been lodged on two primary grounds that are worth discussing here, primarily because I will argue that the criticisms do not apply in the present context. First, Bound, Jaeger, and Baker (1995, henceforth referred to as BJB)

[31] For the purposes of discussion, I consider states whose cutoffs occur on the first day of a quarter to have cutoffs in the previous quarter. These states are Alabama and Arkansas, which have October 1 cutoffs, and Arizona, Florida, and Mississippi, which have January 1 cutoffs. States for which I have entered a "." are states for which I was unable to determine a statewide cutoff date.

[32] Angrist and Krueger (1992) focus on this relationship.

[33] Their study is noteworthy not only because of the novel choice of instruments, but also because its IV estimates exceed its OLS estimates. This finding suggests either that the coefficient on educational attainment in standard wage equations is not affected by endogeneity bias resulting from unobservable ability or that measurement error in reported educational attainment outweighs any such bias.

argue that QOB variables do not satisfy the conditions required of instrumental variables. In particular, they argue that Angrist and Krueger's 2SLS estimates suffer from omitted variable bias. For example, they argue that QOB is correlated with health conditions (e.g. schizophrenia) that plausibly are related to wages outside of any correlation with educational attainment.[34] Thus two stage least squares (2SLS) estimates will confound the structural relationship of interest and the impact of these other, omitted, factors related to both QOB and wages.

The second critique is statistical, rather than substantive, in nature. BJB point out that the reduced form relationship between educational attainment and QOB and SOB variables, i.e. the first stage, in Angrist and Krueger's data is very weak. In finite samples, 2SLS estimates are known to be biased towards OLS estimates, and BJB argue that this bias grows as the first stage relationship – measured by the F-statistic testing joint exclusion of the instruments from the first stage equation – grows weaker. Moreover, the problem is made worse as the number of instruments is increased.[35] Because Angrist and Krueger (1991) use as many as 180 instruments in specifications they report, and because the first-stage F-statistics in these specifications are very low (e.g. 2.43) relative to the rule-of-thumb minimum levels (5 to 10, depending on the number of instruments) suggested by BJB and Staiger and Stock (1997), the validity of Angrist and Krueger's estimates has been challenged.

The 2SLS estimates presented below are not subject to the problems just described, for two reasons. First, QOB variables do an extremely good job of predicting public school enrollment status of five-year-olds. The relevant F-statistics (i.e. those testing joint exclusion of the QOB dummies from the equation predicting five-year-old's enrollment) are in the several hundreds. Second, I focus on specifications using no more than three instruments, dummies for whether a five-year-old was born in the second, third, or fourth quarter of 1974. Thus, neither the weak first-stage nor the many instruments critique applies to the results presented here.[36]

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[34] Research on the role of QOB on psychological disorders includes Rezaul, Persaud, and Takei (1996) and Dassa and Azorin (1993). Demographers and population economists have considered causes of seasonality of birth patterns (e.g. Lam and Miron [1996] and Lam, Miron and Riley [1994]).

[35] A similar point has been made by Staiger and Stock (1997) in a somewhat different econometric framework.

[36] The original Angrist and Krueger (1991) paper spawned something of a cottage industry both in generating alternative estimates of the returns to schooling using QOB/SOB variables and in formulating new estimators designed to overcome the many-instruments and weak-first stage critiques. For example, Staiger and Stock (1997) show that LIML appears to be robust to large numbers of instruments, while Angrist and Krueger (1995) and Angrist, Imbens, and Krueger (1996) construct alternative estimators in the spirit of 2SLS whose purpose is to eliminate the relationship between the instruments and the second-stage residuals. Ironically, these more robust approaches generally yield larger estimates of the education-earnings relationship than the original Angrist and Krueger (1991) ones (though these estimates typically have significantly wider confidence intervals than the original ones). I did compute estimates using several alternative instrumental variables estimators in specifications in which I added 150 QOB×SOB variables to the instrument set. Not surprisingly, given the strength of quarter of birth in

As for the substantive critique lodged by BJB, QOB variables do a similarly good job of predicting five-year-old's enrollment among both mothers whose youngest child is five and those who have both a five-year-old and a younger child (see section 5). However, there is no reduced form relationship between the QOB dummies and outcome measures for mothers whose youngest child is younger than five. This finding supports the validity of QOB instruments because independent (of enrollment) effects of five-year-old's QOB on mother's outcome measures should show up whether or not the mother also has a younger child.[37]

4 Data

I use data from the 1980 5% Public Use Microdata Sample (PUMS). Ideally, I would use more recent data. Unfortunately, the 1990 Census did not ask respondents for information regarding quarter of birth,[38] making the 1980 PUMS the most recent Census data containing QOB.

My main dataset consists of all single mothers[39] in the 1980 5% PUMS who are younger than 50 years old and have one five-year-old child;[40]women in the sample may have any number of children of other ages as well. In order to be five years old on April 1, 1980 (the questionnaire's reference date for the age question) a child must have been born between April 2, 1974 and April 1, 1975. Thus five-year-olds born in the second quarter (April-June) are the oldest ones in my sample. Third-quarter births are next oldest, followed by fourth-quarter births. Five-year-olds born in the first quarter of 1975 are the youngest ones in my sample.

Summary statistics for the final sample, which contains 17,817 women,[41] are reported in Table 1.

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predicting public school enrollment of five-year-olds, the resulting estimates were similar to those generated using only the three QOB main effects, so I do not present those estimates here.

[37] There is still the possibility that the true relationship between children's age and labor market outcomes depends only on the youngest child's age, so that mothers whose youngest child is five respond differently to fine differences in the five-year-old's age than do mothers who have a five-year-old but whose youngest child is younger than five.

[38] The documentation for the 1990 PUMS explains that this change from previous Censuses was made so that the Census Bureau could ask directly for year of birth, which could then be used to check for the accuracy of age data.

[39] I define a single mother as the reference person living in a household described in the PUMS as a "Family household with female householder, no husband present". The sample does contain some women (333 whose youngest child is five and 334 whose youngest child is younger than 5) who report being currently married and not separated. I choose to include these women, since they report that their husbands are not present.

[40] I drop those women having more than one five-year-old in order to maintain comparability. While twins offer another interesting source of variation, fewer than 250 female heads reported having more than one five-year old child.

[41] After dropping observations whose five-year-old had an allocated value for either QOB or type of school enrollment, I found 17,981 female heads who had exactly one five-year-old. Of these, I dropped one observation because the mother was younger than 16 (only those aged 16 and older were asked labor supply questions); I dropped 134 observations because the mother was

In the analysis below, I divide the sample into two subpopulations, for the following reasons. Mothers of five-year-olds whose youngest child is younger than five must still arrange child care for the younger child if they wish to work, even when their five-year-old is attending free public school. By contrast, mothers whose five-year-old is their youngest child and is enrolled need not pay for child care, at least not during school hours. Thus one would expect that women whose youngest child is five respond more strongly to public schooling for the five-year-old. That is, the "treatment effect" of access to public schooling for the five-year-old is likely to differ across the two subpopulations. Stratifying by youngest child's age may allow me to identify the difference in response levels.

However, it is important to realize that child care costs may not be the only differences between the subsamples. By definition, women who have a five-year-old, but whose youngest child is younger than five, must have at least two children. Women whose youngest child is five may have more than one child, but they need not. This difference leads to significant demographic differences between the two subsamples, as I discuss in the next subsection. Perhaps more importantly, I noted earlier that mothers who have a five-year-old but not a younger child have made a decision not to have another child. This decision may be correlated with any or all of the levels of the outcome variables, the decision to enroll the five-year-old in public school, or the treatment effect of access to public school. In fact, as I show in the next subsection, subsequent (of the five-year-old's birth) births to women who have a five-year-old do appear to be correlated with demographic variables (e.g. race).

This discussion suggests that it may be inappropriate to attribute differences in estimated treatment effects only to the different nature of the treatments themselves. That is, estimated differences also may be due to noncomparability of the two subpopulations: the labor supply functions likely differ across the populations, even after appropriate conditioning on the treatment itself. Note that this point does not invalidate the 2SLS results for either population. The question of interest is "How does access to public schooling for her five-year-old affect a woman's labor supply?". As long as the QOB instruments are valid (conditional on any controls necessitated by stratifying on youngest child's age), stratified estimates will be consistent for the effect of "treatment on the treated" (see Manski, 1989) for each subpopulation taken separately. Noncomparability across the subpopulations simply means that the difference in these estimated effects will not identify any interesting difference across treatment effects, since the response functions to given treatment effects differ across these populations.

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older than 50; lastly, I dropped another 29 observations because they reported 9 or more children aged younger than 18, and I lack data on state welfare benefits for family sizes greater than 9 (including the mother). The argument for dropping those with allocated values for QOB is to minimize noise in the first stage. Dropping those with allocated values for enrollment type (the endogenous RHS variable) is necessary to avoid a pitfall specific to measurement error in the case of an endogenous binary variable: Kane, Rouse and Staiger (1998, cited in Angrist and Krueger [1998]) have shown that in this case, instrumental variables estimates will necessarily be upwardly biased.

4.1 Summary statistics by subpopulation

The first subpopulation contains 10,932 observations and consists of mothers whose youngest child is five. The remaining 6,885 observations are single women who have a five-year-old but whose youngest child is younger than five. Table 2 shows variable means and standard errors for women whose youngest child is five. The first row of the table suggests a large role for QOB in determining public school enrollment status of a five-year-old: about four-fifths of the children born in quarters two and three are enrolled, while 55% of fourth quarter births are enrolled and just 31% of first quarter births are enrolled. The second row of the table shows the private school enrollment rate. The private school enrollment rate among fourth- and first-quarter births is relatively higher than that for second- and third-quarter births, reflecting the problems described above.

Women whose five-year-old was born in the first quarter worked significantly fewer weeks and earned considerably less wage income than those whose children were born in later quarters. Mothers of second-quarter births also worked more weeks and earned more than mothers of third and fourth quarter births. Similar, if less impressive, differences are present for the hours, employment, receipt of public assistance, and labor force participation variables.

Notably, the pattern of outcome variables across quarter of birth categories is largely monotonic, with women whose five-year-old was born later working less and receiving public assistance more on average. If child care costs are driving the results I discuss below, then this pattern is to be expected given the monotonic and positive relationship between five-year-old's QOB and five-year-old's access to public school enrollment.

Maximum monthly benefits from AFDC and Food Stamps,[42] in hundreds of monthly current dollars, are quite similar across five-year-old's QOB, varying by only a few dollars across second- and other-QOB five-year-olds. The only variable for which systematic differences occur across quarters is mother's age.[43] Systematic differences of the small degree observed in Table 2 should be expected, since women giving birth in the second quarter of 1974 on average will be older at any point in time than women giving birth three quarters later, in the first quarter of 1975. Since woman's age is correlated with labor supply, this difference across quarters underscores the need to control for mother's age in the regressions reported below.[44] The

[42] Maximum benefits are computed as $A + (F - 0.3A) = 0.7A + F$, where A and F respectively are the state's maximum AFDC payment and the maximum Food Stamps benefit for a family of the appropriate size. The formula is computed as described because the Food Stamps program implicitly taxes AFDC benefits at a 30% rate; see Committee on Ways and Means (1996).

[43] Mother's age is measured in terms of her QOB, though I have presented it in year units. For example, a woman reporting age 29 who was born in the second quarter is assigned the age 29.75.

[44] I tried numerous specifications of mother's age in the results reported below, including quadratic, cubic, quartic, and nonparametric (i.e. including separate dummies for each possible age of the mother) specifications. These modifications had no appreciable impact on the estimated 2SLS coefficient on public school enrollment. I chose to focus on the quadratic specification.

table shows no other apparent systematic differences in observable demographic variables across quarter of birth categories.

Table 3 shows means and standard errors across quarter of birth of the five-year-old among women whose youngest child is younger than five years old. Summary statistics regarding school enrollment for the five-year-old generally mirror those in Table 2, although there are some differences. For example, private school enrollment rates of the woman's five-year-old are significantly lower among women who also have younger children. This difference may reflect both underlying differences in labor market behavior and the lower marginal value to women with children younger than five of enrolling their five-year-old. However, the pattern of public school enrollment across QOB is similar to that observed in Table 2.

There are several notable demographic differences between the summary statistics in this table and those in Table 2. It will be helpful to separate these differences into two categories: differences in levels across the two populations (i.e. across Tables 2 and 3) and differences across QOB within Table 3. I begin with differences across the populations. First, the sample of women with children younger than five is about three years younger on average than those without younger children. Given that by construction these women's youngest child is from 0.25–5 years younger than the youngest child of women in the first group, this fact is not surprising.

Second, by comparison to single mothers whose youngest child is five, single mothers having a five-year-old and a child younger than five have about one more child aged younger than 18. This finding is mechanical: since all women in each sample have a five-year-old child, women with a child aged younger than five must have at least two children. By contrast, women whose youngest child is five need not have another child. The apparent difference in maximum welfare benefits seems also to be an artifact of this mechanical difference in family size. Among women whose youngest child is five and who have more than one child, benefit levels actually are similar to levels among the women summarized in Table 3.[45]

Third, the women having a child younger than five are much less likely to be white.[46] Fourth, women who have a child younger than 5 clearly exhibit much lower levels of labor supply and much greater levels of public assistance participation than women whose five-year-old is their youngest child. This finding may reflect the greater child care costs faced by women having children younger than five. However, it is also likely the result of differences in observable characteristics just described, and any unobservables that are

[45] The average 1979 benefit level among women whose youngest child is five and who have more than one child is $402.

[46] Tabulations of the white dummy among women whose youngest child is five and who have at least two children reveal that no more than three points of the thirteen percentage-point difference between the populations is due to the difference in family size. However, this population is even older, by about a year and a half, than the population shown in Table 2. Thus as a descriptive matter, the difference in racial composition of the subpopulations depicted in the two tables seems to be due to variation by race and age in fertility rates.

correlated with them. Direct effects of the youngest child's age are also a candidate explanation (see, e.g. Angrist and Evans [1998]).

Moving to differences across the five-year-old's QOB, we see that the fraction who are white declines as we move from mothers whose five-year-old was born in the second quarter of 1974 (52% are white) to those whose five-year-old was born in the first quarter of 1975 (only 46% are white). Other examples include the increase in number of children aged 6-12 and the increase in central city residence. These differences across five-year-old's QOB may be partly explained by regional differences in QOB patterns that are also correlated with race, central city residence, and total fertility. An alternative explanation is that different demographic groups exhibit differences in fertility patterns. The cross-QOB patterns observed in Table 3 could be explained by differential birth spacing, while the differences across Tables 2 and 3 could be explained by differential total fertility.[47]

[47] To see the point about spacing, suppose there are two types of women, A and B. Suppose that type A women tend to have children at closer intervals than do type B women. Now suppose that a type A and a type B woman each give birth in, say, the first quarter of 1975. Five years later, on Census Day 1980, the type A woman is more likely than the type B woman to have given birth again, so that she enters the subsample of women with a child younger than five, while the type B woman does not. Now suppose that the two women had instead given birth in the second quarter of 1974 (rather than the first quarter of 1975). Then the type A woman will have given birth by, say the second quarter of 1979, so that again she is in the subsample of women with a child younger than five. This time, however, the type B woman gets a longer period of time (three extra quarters) to "catch up" by having another child.

This discussion suggests that, to the extent that differences in birth spacing occur across demographic groups, the sample of women who have a five-year-old on Census Day and also have a younger child will look more like the overall population of women with a five-year-old on Census Day the longer we wait to compare them; this conclusion is what the variation across QOB in Table 3 suggests. To examine this hypothesis in more detail, I ran some further tabulations of the white and central city dummies among women who had a child younger than five, stratifying by whether the women had a child aged younger than one. Those who did have a child younger than one were much less likely to be white (36% were, compared to 52% of those whose youngest was aged 1–4) and much more likely to live in the central city (44% of those with a child younger than one, compared to 36% of those whose youngest was 1–4). These tabulations suggest at least some role of differential patterns in birth spacing across demographic categories.

The effect of differences in fertility levels operates in the opposite direction on cross-QOB patterns. Suppose that in the example above, each woman has given birth (in the same quarter) so that each has a five-year-old on Census Day. Suppose that more type A than type B women will have another child at some point in the future. Then at the completion of their fertility histories, the population of women with a child younger than the one that is five on Census Day 1980 will have a greater proportion of type A than type B women. Since births happen in any given quarter with probability less than one, differential sorting into the population of women with another child will occur monotonically over time. Thus the more time passes, the greater the difference in the fractions of type A and type B women having another child. Thus mothers of second quarter 1974 births who have another child will look less like the ones who don't have another child, by comparison to all mothers of first quarter 1975 births. Which of the spacing and level effects dominates is clearly an empirical question, but not one that I will try to sort out.

One potential dimension along which fertility patterns may differ is geography. This seems like a promising avenue, since QOB does vary across states. However, as the discussion in the appendix makes clear, alternative reduced form specifications suggest

As discussed in the previous subsection, the cross-population differences suggest that the response (to free child care) functions are likely to differ across the two populations, so that only the within-group effect of treatment on the treated is identified by IV estimates. Cross-QOB differences in the controls present a more difficult problem, since such differences could call into question the legitimacy of the QOB instruments within each population. Looking at the tables of summary statistics, this issue does not seem serious for the population of women whose youngest child is five. For those women who have had another child, my approach will be simply to control for observable demographic variables. While I would of course prefer that this issue had not arisen, I do not believe that the cross-QOB differences in Table 3 are particularly large. Moreover, given the wide array of controls that may be included, my view is that the QOB instruments will be valid once the controls have been included. Moreover, any correlation between the instruments and omitted unobservables may be addressed through overidentification test procedures.

4.2 Effects of QOB on public school enrollment and outcome measures

Table 4 proceeds with the analysis by establishing the first stage relationship between public school enrollment and QOB in a regression framework and by demonstrating a clear reduced form relationship between QOB and labor market outcomes among single women whose youngest child is five. Results reported in Table 5 suggests that among women who also have a younger child, the first stage relationship is present. Labor market outcomes do appear to be related to QOB, but in a haphazard way that does not suggest a role for child care costs.[48] Each table reports OLS regressions of enrollment status and the outcome variables on three QOB dummy variables, demographic control variables, and fixed effects for state of residence and the five-year-old's state of birth.

Turning first to the sample of women whose youngest child is five, we see that children born in quarters 2-4 are much more likely to be enrolled in public school than those born in the first quarter. This effect is about twice as strong for second- and third-quarter births (51.5 and 47.6 percentage points) as it is for fourth-quarter births (24 percentage points). Moreover, these coefficients are approximately the same as simple differences in enrollment rates for children born in the given quarter and those born in the first quarter (see Table 2), underscoring the lack of correlation between QOB dummies and other observable controls in this subsample. Mother's age, age squared, years of education, and central city residence all have small but statistically significant relationships with public school enrollment of the five-year-old. The large

that the demographic control variables discussed in the text, rather than fixed effects for state of residence and five-year-old's state of birth, seem to explain most cross-quarter differences.

[48] The effects of QOB 2-4 are not significant relative to quarter 1. However, differencing the coefficients on the various QOB 2-4 dummies shows that there are significant differences across, say, the second- and third-quarter dummies. This finding simply duplicates those discussed in reference to Table 3 above.

coefficient on race may reflect racial differences in income as well as any cultural differences.

QOB variables also have a clear reduced form impact on the outcome measures among single mothers whose youngest child is five. For example, in 1979 mothers of second-quarter births worked about 2.5 weeks more than mothers of first-quarter births, while about half as large an effect is present among mothers of third- and fourth-quarter births. Employment in 1979 and current employment are significantly greater (by 2.6 and 3.8 points, respectively) among second-quarter mothers than among first-quarter mothers. Weekly hours also respond, with second- and third-quarter mothers working between 0.8 and 1.6 hours more per week, depending on the choice of hours variable and QOB.

Among mothers whose five-year-old was born in the second quarter, wage and salary income for 1979 was significantly greater–by $538–than it was among those whose five-year-old was born in the first. Mothers of third quarter births also had significantly greater wage and salary income. Lastly, having a second quarter birth reduced frequency of public assistance receipt by 2.9 percentage points. In nearly all cases, the QOB effect is monotonic, as should be expected if the effects are due to availability of public schooling.

Table 5 presents analogous estimates for mothers whose youngest child is younger than five. The effect of QOB on enrollment of these women's five-year-olds matches that in Table 4. However, Table 5 shows no consistent relationship of any kind between five-year-old's QOB and mothers' labor supply. The coefficients themselves are almost uniformly statistically insignificant, although their differences are significant in several cases. I interpret the nonmonotonicity of the results as suggesting that the reduced form relationship between labor supply and five-year-old's QOB among women whose youngest child is five (those women in Table 4) reflects child care costs, rather than simply effects due to differences in five-year-olds' age.[49] If women simply worked more when their children got older, such a relationship should also be present among women whose five-year-old is not their youngest child.

5 Effects of Access to Free Public Schooling on Outcomes

In this section, I use QOB dummies to generate instrumental variables estimates of the structural effect of access to free public schooling on the labor market outcomes discussed above: weeks of work in 1979, usual weekly hours in 1979, employment in 1979, wage and salary income in 1979, whether public assistance was received in 1979, hours of work last week, and current employment. In subsection 5.3 I present results for women who are either the reference person or the reference person's wife in married couple households; I present results for such women both when the youngest child is five and when the youngest is younger than five.

[49] See Angrist and Evans (1998) and the discussion in the appendix.

5.1 OLS and 2SLS results

Table 6 presents OLS and Two-Stage Least Squares (2SLS) estimates of equations using weeks of work in 1979 as the dependent variable; only women whose youngest child is five are included in the equations for this table. Means of right hand side variables are shown in Column [1]. Column [2] offers a univariate OLS specification (i.e. no controls are included on the right hand side). The endogeneity and selection problems discussed earlier regarding the public school enrollment dummy are clearly displayed in this column: access to public schooling appears to have a *negative* effect on labor supply, reducing 1979 labor supply by 4.3 weeks of work.

Most of the negative effect can be accounted for by control variables, however, as demonstrated in Column [3]. The regression estimated in this column includes variables controlling for welfare benefit levels, age structure variables for own children and other people living in the household, and mother's years of education, age, race, and central city residence. Fixed effects for current state of residence and the five-year-old's state of birth also are included. The magnitude of the enrollment coefficient falls from about 4 to about 1, while its sign and standard error are unchanged. These results reflect the fact that enrollment of the five-year-old in public school is correlated with other observable variables that independently affect labor supply.

Column [4] presents Wald (1940) estimates of the same model. The Wald estimator is a special case of 2SLS with one endogenous right hand side variable, one binary instrument, and no other right hand side variables. The estimator is defined by separating observations according to their value for the instrument. Then one constructs differences in the means of the dependent variable of interest to the endogenous variable and takes the ratio of these differences, yielding the estimate. Thus the Wald estimator is simply the ratio of the coefficient on the instrument in the reduced form equation of interest to the coefficient on the instrument in the first stage.[50] The estimator is useful in generating simple instrumental variables estimates that may

[50] Let y, x, z be, respectively, the (demeaned) vectors of observed values of the outcome variable, the RHS endogenous variable, and the instrument. In the present case, y_i is some labor supply measure for person i, x_i is a dummy for public school enrollment of the five-year-old, and z_i is a dummy for whether the five-year-old was born in the first quarter. Then the 2SLS estimator is exactly identified and so can be written as $\beta_{2SLS} = (z'x)^{-1}z'y$. Since each of z, x, y has dimension 1, we can rewrite this formula as

$$\beta_{2SLS} = \frac{z'y}{z'x}.$$

But if we divide both numerator and denominator by $z'z$, we see that we have the ratio of the two reduced form estimators. In turn, since in its non-demeaned form z_i is just a dummy variable, these reduced form estimators themselves are just differences in means according to the value of z_i:

$$\beta_{2SLS} = \beta_{Wald} = \frac{\bar{y}|_{z=1} - \bar{y}|_{z=0}}{\bar{x}|_{z=1} - \bar{x}|_{z=0}},$$

which gives the result.

be compared to their simple OLS counterparts (e.g. the estimate presented in Column [2] of Table 6).

The Wald estimate in Column [3] dramatically illustrates the effect of endogeneity and selection in determining public school enrollment: the estimate implies that access to public schooling actually leads to an increase in labor supply of 4.7 weeks of work. Moreover, this estimate is highly precise.

Column [4] departs from the Wald specification by adding all controls used in Column [3] and using all three QOB dummies as instruments for enrollment of the five-year-old. The Column [4] estimate of 3.6 weeks is notable for several reasons. First, the estimated effect is statistically very significant–nearly four times its standard error.

Second, the estimate is economically significant. Predicting weeks of work using the Column [4] coefficients but setting the enrollment dummy equal to 0 for all sample members yields an average prediction of 26.1 weeks worked. Setting the enrollment dummy equal to 1 raises this average prediction by the size of the Column [4] coefficient, for a new predicted average of 29.7. Thus providing access to public school enrollment for the five-year-old raises average weeks of work by 13.8%.

Third, unlike the OLS results, the 2SLS results are insensitive to the addition of controls: the reduction in the estimated coefficient on the public school enrollment dummy, from 4.7 to 3.6 is well within the range of reasonable sampling error.[51]

Coefficient estimates for the other variables in these equations are consistent with previous labor supply studies and generally confirm predictions from economic theory.[52] Race, age, age squared, the central city residence dummy, and education of the mother all have signs typically observed in labor supply studies.

The interpretation of the children's age variables is somewhat subtle. Having another child aged 6-12 significantly reduces weeks of work (the coefficient in Column [5] is nearly -3), which should be expected, since these children will require after school child care.[53] Adding an own child aged 13-17 to the woman's household appears to have no effect on weeks of work, as its Column [5] coefficient is just -0.14. The number of own children aged 18 or older in the household also has a tiny impact on weeks of work.

However, it would be incorrect to say that the 13-17 or 18+ categories are not important, since these coefficients are estimated conditional on the number of children in other age categories. Consider the impact

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[51] It should be noted that the instrument set is different in the two cases, but eliminating this difference has little effect.

[52] It is interesting to observe that instrumenting for the enrollment variable has little impact on the coefficients of other variables included in the Column [3] OLS specification. This result underscores the fact that quarter of birth variables have little correlation with the controls (as Tables 2 and 3 demonstrated).

[53] Of course, family structure may be endogenous. If so, then household age structure variables will pick up both the child care-related effects of household age structure and the correlation between these variables and unobservable characteristics of the mother.

of having a twelve-year-old instead be a thirteen-year-old: weeks of work would increase by 2.6 (having one less twelve-year-old increases labor supply by 2.76, while having one more thirteen-year-old reduces labor supply by 0.14 weeks). If instead, the twelve-year-old were replaced by an eighteen-year-old, weeks of work would increase by 2.98. Thus it is important to know not only a woman's number of children, but also their age structure.

Similarly, one needs to know not only the number of other household members, but also their ages. The number of other household members aged 18 or greater was associated with an increase in weeks of work of about 2. This relationship is to be expected, because these family and household members may be able to care for young children. The number of other household members aged younger than 18 almost exactly offsets the number of adult other household members, possibly because they add to household child care costs for women who wish to work. Thus having two additional household members could be associated with an increase in labor supply of four weeks, a reduction of four weeks, or no change, depending on the new household members' ages.

The coefficient on the level of AFDC plus Food Stamps benefits, measured in hundreds of dollars, is -2.68 and is significant at the five percent level. One unit of standard deviation for the estimation sample in Table 6 is $113, so raising maximum benefits by one unit of standard deviation is associated with a reduction in labor supply of 3.04 weeks, or about 10.7%.[54]

Table 7 reports the coefficients on the public school enrollment dummy obtained by estimating the specifications just described for all seven outcome measures among women whose youngest child is five.[55] The results reported in the table establish that access to free public schooling has an unambiguously significant effect on labor market outcomes for women whose youngest child is five. Moreover, the pattern across specifications mirrors that seen in Table 6: the simple OLS estimate is wrong-signed and large in magnitude; including controls greatly reduces the estimated magnitude, but without changing its sign; the Wald estimates are large and of the expected sign; and the Column [4] estimates using the controls and all three QOB dummies as instruments are similar to the Wald estimates.[56]

[54] Because I use state fixed effects in all specifications including benefit variables, the coefficient on benefits is identified only by differences in family size. More precisely, the coefficient is identified by cross-state variation in differences in benefit levels by family size. The state fixed effects rule out any problem with equicorrelation of unobservables at the state level (i.e. as discussed by Moulton (1990)); I have not explored whether there remains equicorrelation by family size or state-by-family size groupings.

[55] The pattern of estimated effects for the controls in the equations for the six other outcome measures generally is very similar to that for weeks of work. Full tables of these estimates are available on request.

[56] I discuss more formal statistical results, based on Hausman test procedures, concerning the similarity of the Column [3] and Column [4] results in the subsection on instrument validity, below.

In order to obtain an economically meaningful idea of the magnitude of the estimated effects, it will be useful to normalize the estimated effects by the means of the dependent variables when these are predicted with the enrollment indicator set to 0. The result is the percentage change in the dependent variable that the model predicts by comparing the hypothetical world in which there is universal public school access for all mothers in the sample with the world in which no one has access. Column [5] of Table 7 presents these predicted values. Standard errors for the predicted means were computed using the delta method.[57]

Estimated percentage effects are presented in Column [6] of Table 6. These estimates imply that public schooling increased 1979 weeks of work by 13.8%; increased usual hours for 1979 by 9.2%; reduced the probability of welfare receipt by 11.9%; increased wage and salary income by 20.2%; increased hours worked in the week before filling out the Census questionnaire by 13.7%; and increased current employment by 9.2%. In each case, the estimated percentage effect is significantly different from zero.[58]

There are several things to note in these results. Both employment and usual hours in 1980 appear to respond by more than for last week. However, these differences are not statistically significant, partly because the estimates for the 1980 variables have substantially larger standard errors than those for the 1979 variables.[59]

Second, wage and salary income responds a good deal more than the labor supply variables. Two possible explanations for this finding come to mind. First, women receiving child care through public schooling may be able to take jobs with longer hours, higher wages, or both. For example, a woman working in only part-time jobs is likely to earn less per hour than a woman working full-time; unless she works more than one part-time job, she will also work fewer hours than she would if she worked full-time. If there are fixed costs to working, or working full-time, then the number of hours of child care provided by public schooling need not be large to induce this sort of effect.

A different, but perhaps complementary, explanation is that women who receive free child care through public schools may be more likely to report labor income. Since various components of the welfare system impose large implicit taxes (sometimes exceeding 100 percent) on labor income, participants have an incentive

[57] Let x be the values of the RHS variables used for the prediction, and let $\hat{\beta}$ be the estimated coefficients for these variables. Then the predicted mean is $x\hat{\beta}$, and its variance is $V[x\hat{\beta}] = x\ V[\hat{\beta}]x'$. In this case, the delta method is simply the standard formula for computing the variance of a linear combination of estimated statistics.

[58] Standard errors for the percentage effects were also computed using the delta method. The estimated percentage effect is $f(x\hat{\beta}) = \hat{\beta}_{public}/(x\hat{\beta})$, where $\hat{\beta}_{public}$ is the estimated coefficient on the public school enrollment dummy. Then the formula for the variance is $\nabla V[\hat{\beta}]\nabla'$, where ∇ is the vector of first partial derivatives of the percentage effect with respect to all the estimated coefficients.

[59] These larger standard errors should be expected, since current employment and hours last week are noisy measures of the same variables over a year's time.

not to report income to the government. It is well-known that reported consumption in household surveys among welfare participants is far greater than reported income (see Edin and Lein [1997]), suggesting that women who earn income under the table may be hesitant to report that income to survey-takers.[60] If access to free child care causes women not to participate in welfare programs, they may be more willing to report income they would have earned even while participating in such programs.

Third, as noted earlier, the results for 1979 variables are derived from provision of free public schooling only between the beginning of the school year in early September and the end of the calendar year. One way to correct for the underestimate would be to scale the 1979 variables by the fraction of the school year during which free public schooling was likely to be available for five-year-olds.

Assuming 17 weeks of free schooling suggests a scale factor of approximately 3. This scaling would lead to enormous effects–for example, an increase in yearly weeks of work of over 40 percent. However, one would also have to triple the standard error on the estimated percentage effect. Moreover, it is possible that women might take jobs in, say, July, knowing that they will have free child care beginning in September, in which case a scale factor of 3 would be too large. Nonetheless, the estimates presented in this subsection certainly would seem to understate the actual effects that should be expected under provision of free, year-round child care.

Another approach, at least for the employment and hours variables, is simply to compare the estimated percentage effects (or the estimated coefficients) for the 1979 variables to those for current employment and last week's hours. Such a comparison is complicated somewhat by the fact that common data are used to estimate the coefficients for each outcome variable, while I did not estimate the equations jointly (e.g. with a seemingly unrelated regressions approach). Thus while it seems unlikely that the estimated effects are independent of each other, I have no estimate of the covariance of the effects. One approach would be to estimate the pairs of equations jointly. However, for both employment and hours, the differences in both the coefficients and the percentage effects are small enough that it seems unlikely one could reject that they are the same. On the other hand, the point estimates for the 1979 variables are smaller than the corresponding 1980 estimates (whether one considers the coefficients or the percentage effects).

I also computed results for female heads whose youngest child was younger than their five-year-olds. These results are reported in Table 8 in a format analogous to that in Table 7. As in Table 7, the simple OLS results in Column [1] are significant and wrong-signed. Adding the contols, in Column [2], drives the estimated coefficients toward zero and reduces their significance, in some cases below conventional levels for rejecting a zero effect. The Column [3] Wald estimates are properly signed but uniformly insignificant. Moving to Column [4], we see that adding the controls and using 3 QOB dummies as instrumental variables

[60] In fact, Edin and Lein's (1997) informal survey of low-income women finds that nearly all welfare recipients have at least some labor income that they do not report to their caseworkers.

leads to uniformly insignificant results and no clear pattern in the sign of the estimated effects.

5.2 Instrument validity

In assessing the results reported in this section, it is important to remember that their validity hinges on the validity of excluding the QOB dummies from the second stage (i.e. the outcome equations of interest). A simple alternative hypothesis seems plausible: QOB may affect mother's outcomes outside of its effects on five-year-olds' public school enrollment because QOB is directly (and linearly) related to five-year-olds' age. Angrist and Evans (1998) find that, among women aged 21–35 who have at least two children, presence of a third child reduces mother's labor supply. More importantly for present purposes, the effect declines with the third child's age until the third child turns 13, so that women work more as their children age.

While it is impossible to test whether or not each of the exclusion restrictions are individually valid, overidentification tests allow one to examine the hypothesis that they are jointly satisfied in a straightforward way. Thus I report in Column [5] of Table 7 a χ^2 overidentification test statistic. This statistic is computed by regressing the predicted residuals from the second stage on all the exogenous variables (i.e. the controls as well as the instruments). The product of the number of observations and the R^2 from this regression yields a statistic that is asymptotically distributed as χ^2 with degrees of freedom equal to the number of instrumental variables minus the number of endogenous variables.[61] If my results are driven by such an age-of-five-year-old effect, then this test statistic should differ greatly from zero, suggesting that the variables treated as exogenous jointly have too much power in explaining second stage predicted residuals.

Column [7] of Table 7 suggests that among single women whose youngest child is five, the only variable for which the overidentification test suggests any problems at conventional levels is weeks of work in 1979. Since there is no obvious reason why the QOB dummies should be good instruments for the other dependent

[61] In the present case, there are three QOB dummy instrumental variables and one endogenous variable. The test is justified under the usual generalized method of moment conditions, provided that the estimator in use is asymptotically efficient (see Greene [1993]).

variables of interest but not for weeks, little additional comment seems necessary on this issue.[62,63] Results in Column [7] of Table 8 are less comforting: in two cases, the p-value is less than 0.05, and in two others it is less than 0.11. Hence the validity of the instruments among women whose youngest child is younger than five is called into some doubt.

An additional, and complementary, way to examine the validity of the instruments is to compare estimates of the public school enrollment effect generated with and without inclusion of demographic controls and state fixed effects. Rigorous testing can be based on the idea that if the instruments are valid without conditioning on such variables, estimates generated with and without those variables should not differ systematically. Under the null hypothesis that the instruments are unconditionally valid, the 2SLS estimator that restricts the effects of the controls to be zero will be both consistent and efficient. Under the alternative hypothesis (of either invalidity or only conditional validity of the QOB instruments), this estimator will be inconsistent. Under either hypothesis, the 2SLS estimator that does not restrict the effects of the controls will be consistent.

Comparisons of the Column [4] QOB Column [3] Wald estimates from Table 7 suggest little reason for concern: p-values for rejecting unconditional instrument validity (relative to the controls) were above 0.15 for all left-hand side variables except for current employment; that p-value was 0.08. It should be noted

[62] However, as a further test, I examined the reduced form relationship between QOB dummies and mother's labor supply for a sample of single mothers whose youngest child was three years old (the criteria for inclusion in this sample were otherwise identical to criteria for inclusion in the main sample discussed in that text). I chose three-year-olds rather than four-year-olds because relatively few three-year-olds (12.8%) in this sample were reported enrolled in public school (compared to 27.9% of four-year-olds). It is not clear what kind of programs these children are attending. One possibility is Head Start; also, states and local areas may provide pre-kindergarten education, although I am unable to observe access to such programs when children are not enrolled in them. While the QOB dummies have a small and statistically significant impact on the likelihood of three-year-olds' public school enrollment (e.g. second quarter births are 10% more likely to be enrolled than first quarter births), they have no impact whatsoever on the labor market outcomes considered here. In no case is any QOB dummy statistically or economically significant in predicting labor supply, earned income, or receipt of public assistance among single mothers whose youngest child is three.

[63] A potential reason for QOB dummies to fail the exclusion restriction for the weeks variable has to do with age of the five-year-old. The weeks variable measures participation decisions both before and after free child care via public school becomes available in September. Thus if age of the child affects participation independently of child care costs at ages younger than the average age of five-year-olds in the sample in September, QOB dummies could be correlated with weeks between, say, January and August. Participation variables are less likely to be subject to this problem, since their binariness allows inherently less variation over the year. However, I find this explanation unlikely, since the estimates for wage and salarly income and usual hours in 1979 each pass the overidentification test despite being similar to the weeks variable along this dimension.

that the instrument sets are different in Columns [3] and [4].[64] I therefore computed estimates[65] with 3 QOB dummies as instruments but with no controls included and then ran Hausman tests of these estimates relative to those in Column [4] of Table 7. In all cases except for 1979 wage and salary income, the p-values for rejecting instrument validity exceeded 0.2; for wage and salary income the p-value was 0.09. Taken together, I believe that these results suggest one should have a large degree of confidence in the estimates presented in Column [4] of Table 7.

Analogous Hausman test results for the women in Table 8 again suggest problems with the QOB instruments. Regardless of whether the Wald or alternative QOB estimates were compared to the estimates in Column [4], one can reject unconditional instrument validity with high levels of confidence. In only one of the 14 tests (comparison of the Column [3] Wald and Column [4] estimates for employment in 1979) did the p-value exceed 0.05, and in that case it was 0.06. I conclude that there is reason to doubt the validity of the QOB instruments among single women whose youngest child was younger than five.

To summarize this subsection, there is strong evidence for the validity of the QOB instruments among women whose youngest child is five. There is equally strong reason to believe that the instruments are not valid among women whose youngest child is younger than five.

5.3 Married women

In this subsection, I briefly discuss results generated using married women living in married couple families. A woman is included in the married sample if (i) she is either the household reference person (and her husband lives in the household) or if she is the wife of the reference person, and (ii) she meets all sample selection criteria described for single mothers above. The sample of married women includes 54,868 women living in married couple families whose youngest child was five, and 53,732 who had a five-year-old and a younger child (as before, women in the sample could have any number of children not aged 5).[66]

[64] Asymptotically, this fact should have no impact on the Hausman test statistic under the null hypothesis that (all of) the instruments are valid, since alternative IV estimates will be asymptotically equivalent if the true treatment effect is itself constant throughout the population represented by my sample (an alternative interpretation of IV estimates, under which different instruments will be associated with different underlying treatment effects) is presented by Angrist and Imbens (1994).

[65] To conserve space, I have omitted these estimates from the paper; they are available on request.

[66] In the interest of conserving space, I have not included tables of means for these two groups. As with female heads, means of demographic control variables (other than mother's age) show no systematic variation across five-year-old's QOB for either population. Patterns of enrollment rates in public school were generally similar to those for female heads, although fourth- and first-quarter birth five-year-old children of married women were substantially less likely to be enrolled in public school in public school. For example, among female heads whose youngest child is five, 55 percent of fourth quarter births and 31 percent of first quarter births were enrolled in public school. Among married women, the corresponding figures were only 44 and 20 percent. There was virtually no difference in five-year-old's public school enrollment status (either across quarters or overall) between

Tables 9 and 10 present estimates from OLS and 2SLS regressions analogous to those considered in Tables 7 – 8: results in Table 9 are for married women whose youngest child is five, while those in Table 10 are for women whose youngest child is younger than five. The OLS results in Tables 9 and 10 generally would suggest that labor supply of married women either does not respond to access to public school for the five-year-old or responds negatively. However, both the Wald and full 2SLS estimates suggest that while there was no impact on receipt of public assistance or wage and salary income, access to public school for the five-year-old did increase married women's labor supply. There is little difference in the size of the responses between married mothers whose youngest child is five and those whose youngest is younger than five. For example, access to enrollment increased current employment by 8.8 percent among mothers whose youngest was five and by 9.0 percent among mothers whose youngest was younger than five. With the exception of wage and salary income, it is not possible to reject that the percentage response of either population of married mothers differs from the response of female heads whose youngest child is five (although in the case of public assistance receipt, this conclusion is forced by the extremely large standard errors for the percentage response of married mothers, who are categorically ineligible for many welfare programs).

6 A rough cost-benefit analysis

Fortunately, a rough cost-benefit analysis does not require accurate estimates of the full-year treatment effect. The Department of Education provides statistics on average expenditures per day per pupil in attendance in U.S. public schools. For the 1979-1980 academic year, U.S. schools spent an average (in current dollars) of $2,491 per pupil in attendance (see U.S. Department of Education [1980]).[67] Estimated effects for 1979

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married women whose youngest child was five and those whose youngest was younger than five. However, both populations of married women enrolled their five-year-olds in private school more frequently than did female heads, and there is a clear increase across the quarters in private school enrollment rates. For example, among married women whose youngest child is five, 14 percent of second quarter births were enrolled in private school, compared to 32 percent of first quarter births; among married women whose youngest child was younger than five, these figures are 14 and 28 percent, respectively. Full tables of these summary statistics are available on request.

Among married women whose youngest child is five, average labor supply and earned income are about 25 percent lower than among female heads whose youngest is five. These averages are also lower among married women whose youngest child is younger than five compared to levels for the corresponding sample of female heads. Among both populations of married mothers, receipt of public assistance is nearly zero, suggesting that most public assistance reported in the female heads sample likely involves programs with categorical eligibility tests that exclude married couples.

I also estimated reduced form regressions analogous to those presented in Tables 4 and 5 (these results are also available on request). Estimated coefficients from these regressions suggest a definite impact of five-year-old's QOB on outcomes among both populations of married women.

[67] This figure includes expenditures on people costs, capital outlay, and interest on school debt, all of which are relevant costs for considering the long-run cost of providing schooling.

variables result from provision of free public schooling from the beginning of the school year through the end of the calendar year. Assuming that the school year begins on September 1 and lasts ten months yields a total schooling cost for Fall 1979 of $\$2,491 \times 0.4 = \$996.40$.

This figure may misstate the relevant cost for three reasons. First, children in kindergarten often attend only part of the day, suggesting that the figure overstates the cost of providing kindergarten. Second, and potentially more importantly, the costs of expanding access to public schooling to children younger than those of kindergarten age may be quite different from the average cost of educating children currently in school. It seems unlikely that kindergarten class sizes would be deemed appropriate for younger children, so that more staff would be required. One would also like to determine whether provision of kindergarten costs more or less than provision of the average education for given numbers of hours and class sizes. Third, providing schooling for children of single mothers might cost more than the observed average, if these children are more difficult to care for.

As for benefits, there is no obvious way to assess the value to the children themselves of the school time they actually receive.[68] However, one can determine the increase in mother's income expected as a result of access to free child care. It is important to remember that some of this income is returned to various levels of government through the tax system. Determining how much is not easy, however. Because average earnings in the sample are so low—about $7,591 among those women with positive earnings—it is hard to know what the appropriate marginal and average tax rates should be. Moreover, the Earned Income Tax Credit would have to be taken into account. Another problem arises because added earned income will reduce public transfer receipts for women on a variety of programs. Without knowing more about which programs sample members participate in, it is impossible to compute the savings to any of these programs.

In any event, added tax liabilities and reduced public assistance payments simply represent transfers from sample members to various governmental entities. Thus estimated increases in labor income may still be used for cost-benefit purposes, so long as it is understood that the results of such analysis refer to total benefits; one cannot draw conclusions regarding the benefits specifically received by single mothers and those specifically received by taxpayers via the government.

Lastly, if some of the increase in labor income is due to nonreporting of income by women without access to free child care, then three modifications of the analysis are necessary. First, offering care will increase tax revenues by more than the appropriate share of the increase in actual income, since inframarginal income will now be reported. Second, public assistance payments will also decline by more than what they would under the assumption that all labor income increases are real, rather than reporting-based. Third, disposable

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[68] Mayer (1997) argues that facets of childhood other than household income, e.g. the quality of educational experiences a child has, have a much larger impact on children's outcomes than does income. Thus the figures relating to mother's income described in the text above may fail to capture an important source of benefits to children themselves.

income of single mothers will rise by less than the estimate from Table 7.

With all of these caveats, any cost-benefit analysis is necessarily tenuous. However, one can gain a rough idea using the $996.40 cost figure and the estimates reported here. The results in Table 7 suggest that among single mothers whose youngest child is five, access to free schooling raises labor income by an average of $929.15 per mother in the sample. Using this estimate, each dollar spent providing public schooling for a five-year-old who is her mother's youngest child returns $0.93 in additional labor income earned by the mother. Given the current desire of policymakers and voters to move single mothers off of welfare and into the workplace, school-based child care seems like a very good deal. These findings confirm the view that provision of free child care (of kindergarten-quality) likely is a relatively cost-effective way of redistributing resources to single mothers while increasing their labor supply, provided that child care is available for all of a mother's preschool-aged children. By contrast, the results in Tables 9 – 10 suggest little impact of public school access on the wage and salary income of married mothers, even as it increases their labor supply.

7 Conclusion

In this paper, I have argued that access to free public school for a woman's five-year-old has a large impact on the child care costs she must bear in order to work. OLS estimates of the effect of five-year-old's enrollment status on single mothers' labor supply suggest a large degree of endogeneity of enrollment. A variety of instrumental variables estimates provide evidence that access to public schooling for the five-year-old has a strong effect on labor market outcomes among mothers whose youngest child is five years old. No effect is apparent among mothers of five-year-olds whose youngest child is younger than five, supporting the view that the marginal child care effect is being identified among women in the first group. Among married women, effects generally were statistically and economically significant. Access to public schooling for the five-year-old had no impact on outcomes for single mothers who also had at least one child aged less then five.

Results from a back-of-the-envelope cost-benefit analysis suggest that direct provision of child care may be an efficient way of redistributing resources and increasing labor supply of single mothers: each dollar spent on provision of public schooling appears to generate $0.93 in additional labor income for a single mother whose youngest child is five. Some part of this increase will be returned to governments via tax revenues and reduced expenditures on public assistance. Also, the figure may overstate the gain in labor income due to changes in reporting of earned income as earned income rises and welfare participation falls. On the other hand, this figure does not account for the value of the education received by five-year-olds enrolled in public schooling.

In any case, the results reported here clearly imply that the provision of free public schooling has a large and statistically significant effect on the labor market outcomes of single women whose youngest child

is of kindergarten age. Existing studies of child care costs and female labor supply commonly make the assumption that provision of a given level of child care quality has similar effects on labor supply, regardless of children's ages. Under this assumption, my results suggest that subsidies for child care expenditures or universal provision would have a large and positive impact on labor supply and wage income among single mothers, while reducing welfare participation. Even without the assumption, however, it seems likely that child care subsidies would have a similarly advantageous impact among women whose youngest child is, say, four years old.

Appendix

A potential source of concern is that, when QOB comparison groups are chosen selectively, outcome variables appear to respond to QOB. For example, looking at Table 3 we see that mothers of first quarter (1975) births reported usual hours of 15.27, while mothers of second quarter (1974) births reported 17.42; the difference was statistically significant at the .05 level. Moreover, this difference was relatively larger than the corresponding difference among mothers whose youngest child was five (i.e. those described in Table 2).

At first glance, a similar critique might seem appropriate for the reduced form results in Table 5 below, since the inclusion of demographic controls seems to have eliminated the significance of the QOB dummies, and if QOB is uncorrelated with other demographic variables, then including such variables should have no impact on coefficients of QOB dummies. However, comparisons between Table 3 cross-quarter differences and Table 5 coefficients is inappropriate, because Table 5 regression coefficients effectively measure the difference between first- and other-quarter effects. Thus one needs to difference the Table 5 coefficients for, say, the second- and third-quarter dummies in order to compare differences for the second- and third-quarter columns in Table 3 to results in Table 5. Much of the pattern of differences in Table 3 is replicated in Table 5 when one measures differences this way.

However, some differences still occur across the tables (e.g. the coefficient on the second-quarter dummy in the Table 5 weeks equation is significantly lower in magnitude than the relevant Table 3 difference). The argument in the text about differential fertility behavior suggests that conrolling for demographic characteristics and state of birth should get eliminate some of these differences. To try to isolate the reasons for cross-QOB differences in the outcomes, I therefore ran several alternative specifications of the reduced form regressions reported in Tables 4 and 5. First, I regressed the seven outcome variables on the QOB dummies, with no other variables included; I ran these specifications for both populations of women (i.e. those whose youngest child was five and those whose youngest was younger than five). Second, I added fixed effects for state of residence in 1980 and five-year-old's state of birth. Third, I dropped those fixed effects, instead including only the three QOB dummies and the demographic controls included in Tables 4 and 5.

Among women whose youngest child was five, the results varied little, either in statistical significance or the pattern of estimated coefficient values, across reduced form specifications. By contrast, the significance of the estimated coefficients for women whose youngest child was younger than five did appear to vary across specifications. For the regression with neither fixed effects nor demographic controls, coefficients on the second quarter dummy were statistically significant at the .05 level for weeks of work in 1979, employment in 1979 and current employment; the coefficient on the second quarter dummy was significant at the .10 level for the usual hours equation. However, in general, the nonmonotonic relationship between QOB dummies and outcome variables observed in Table 3 persisted.

When I added the fixed effects, the second quarter coefficient for the usual hours equation lost its significance, and the second-quarter dummy coefficient values fell in magnitude for the other equations. However,

the estimated coefficients for these dummies were clearly within sampling variance of those estimated in the QOB-only specification.

Adding the demographic controls in lieu of the fixed effects eliminated all statistical significance of the QOB dummies (though the third quarter dummy for the current employment equation became negative and significant at the .05 level). Again, most of the change appears to have come from the coefficient levels rather than the standard errors. However, the estimated coefficient levels remain within sampling variance of those from the earlier specifications. Moreover, comparisons of the QOB dummies in the reduced form equations with demographic controls but no fixed effects and those that include fixed effects as well suggest little impact of the fixed effects. That is, the results presented in Table 5 are insensitive to exclusion of the fixed effects, but not exclusion of the demographic controls.

Thus it appears that differences between Tables 3 and 5 occur because of the demographic controls, although the difference could be due to sampling variance. While I would obviously prefer that the controls played no such role, I do not believe that this issue presents a serious problem. First, if child care costs and public school enrollment were driving the differences in outcomes, then these differences should be monotonic across quarters. While that hypothesis is confirmed for mothers whose youngest child is five – regardless of the reduced form specification I use – it clearly is rejected for those whose youngest is younger than five.

Second, regardless of specification, at most five of the 21 QOB dummy coefficients are ever significant among mothers whose youngest child is younger than five (again, though, it is important to remember that the QOB dummies in reduced form specifications measure only differences between the quarter in question and the first quarter). Third, there is little *a priori* reason to believe that public school enrollment and QOB dummies should have no effect on outcomes among women whose youngest child is younger than five; rather, one simply expects these variables to have a smaller effect in this population than among women whose youngest is five. Lastly, I ran several 2SLS regressions of the seven outcomes on the public school enrollment dummy, instrumenting with three QOB dummies. I tried including as exogenous right hand side variables no controls at all, only fixed effects, only the demographic controls, and the combination of fixed effects and demographic controls. The 2SLS coefficient on the enrollment dummy was smaller in magnitude than its standard error in most cases, and the coefficients' sign tended to bounce around. I conclude that there is little evidence that female heads whose youngest child is five respond to access to public schooling for their five-year-old.

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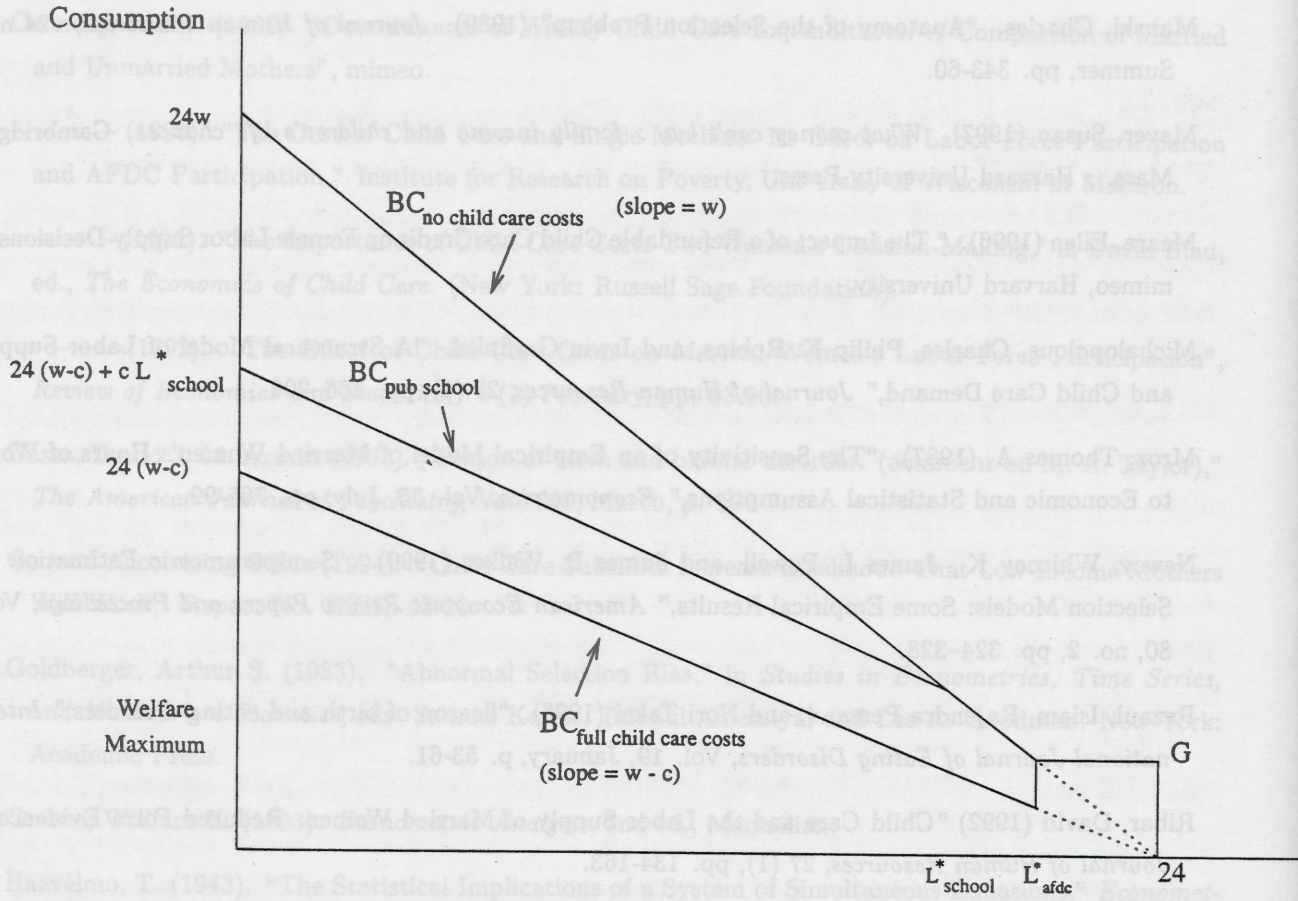

Figure 1: Effect of free, inframarginal child care on budget constraint

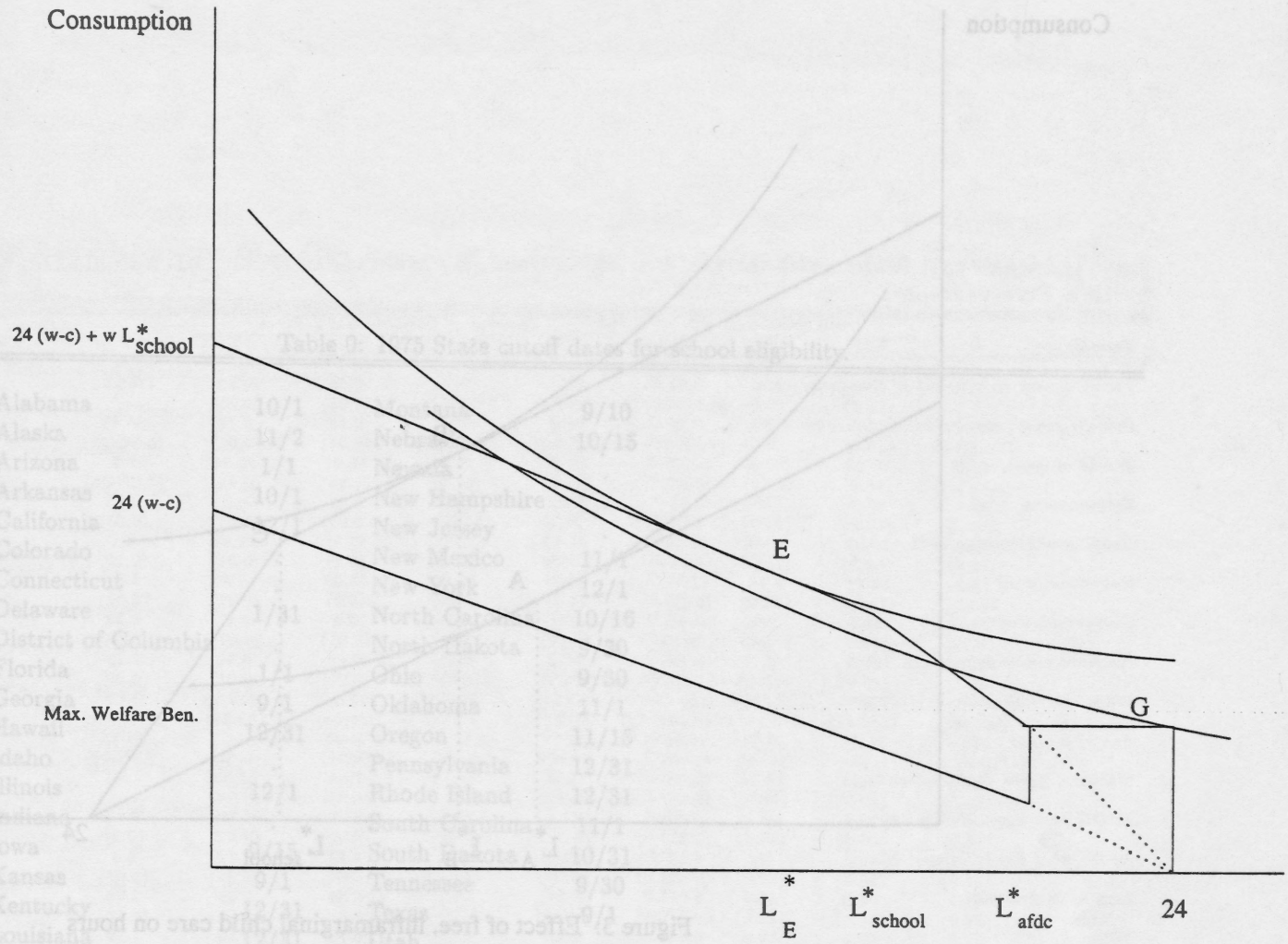

Figure 2: Effect of free, inframarginal child care on participation

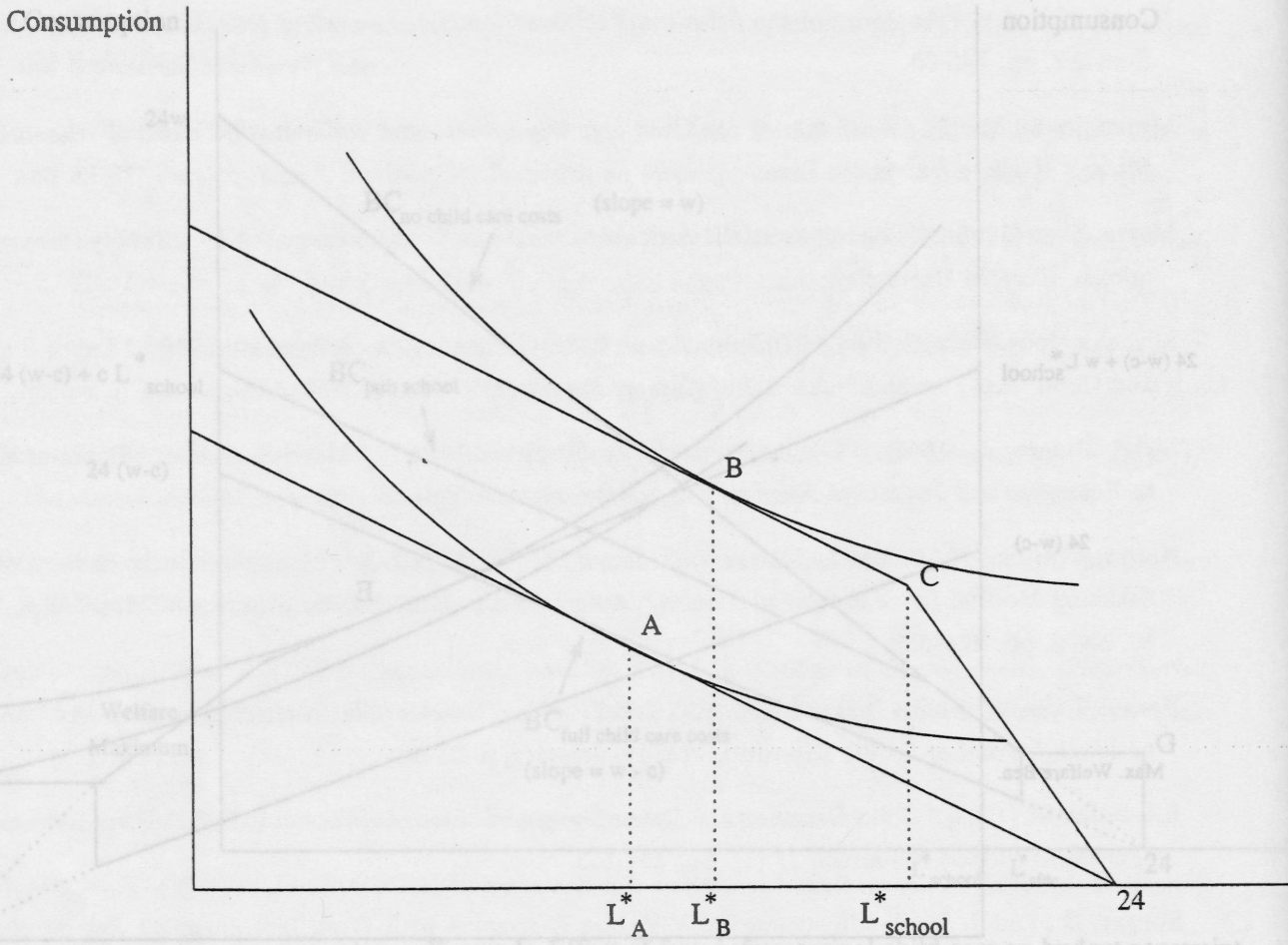

Figure 3: Effect of free, inframarginal child care on hours

Table 0: 1975 State cutoff dates for school eligibility.

Alabama	10/1	Montana	9/10
Alaska	11/2	Nebraska	10/15
Arizona	1/1	Nevada	.
Arkansas	10/1	New Hampshire	.
California	12/1	New Jersey	.
Colorado	.	New Mexico	11/1
Connecticut	.	New York	12/1
Delaware	1/31	North Carolina	10/16
District of Columbia	.	North Dakota	9/30
Florida	1/1	Ohio	9/30
Georgia	9/1	Oklahoma	11/1
Hawaii	12/31	Oregon	11/15
Idaho	.	Pennsylvania	12/31
Illinois	12/1	Rhode Island	12/31
Indiana	.	South Carolina	11/1
Iowa	9/15	South Dakota	10/31
Kansas	9/1	Tennessee	9/30
Kentucky	12/31	Texas	9/1
Louisiana	12/31	Utah	.
Maine	10/15	Vermont	11/30
Maryland	.	Virginia	12/31
Massachusetts	.	Washington	12/31
Michigan	12/1	West Virginia	9/1
Minnesota	9/1	Wisconsin	12/1
Mississippi	1/1	Wyoming	9/15
Missouri	9/30		

Note: Children who are five years old on April 1, 1980, are eligible to begin school in 1979 if they were born before the cutoff date in 1974 if the cutoff date occurs in quarters 2-4 or the cutoff date in 1975 if the cutoff occurs in quarter 1.

Table 1: **Means and Standard Errors of Variables Used, By Quarter of Birth (All Female Heads With a Five-year-old)**

Variable	Full Sample	*Quarter of five-year-old's Birth* 74:II	74:III	74:IV	75:I	F
Public school enrollment of five-year-old	0.64	0.84	0.8	0.56	0.32	1359.22
	(3.6E-03)	(0.01)	(0.01)	(0.01)	(0.01)	
Private school enrollment of five-year-old	0.13	0.09	0.11	0.15	0.18	60.25
	(2.5E-03)	(4.4E-03)	(4.4E-03)	(0.01)	(0.01)	(9.5E-39)
Weeks of work, 1979	23.57	24.67	23.3	23.91	22.35	7.9
	(0.17)	(0.35)	(0.32)	(0.35)	(0.35)	(2.9E-05)
Employment, 1979	0.62	0.63	0.62	0.62	0.6	2.98
	(3.6E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)
Usual weekly hours, 1979	22.49	23.12	22.42	22.57	21.82	3.1
	(0.15)	(0.3)	(0.28)	(0.3)	(0.31)	(0.03)
Hours last week	17.72	18.25	17.69	18.03	16.86	3.89
	(0.15)	(0.3)	(0.29)	(0.3)	(0.31)	(0.01)
Current employment	0.49	0.51	0.48	0.5	0.47	5.52
	(3.7E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(8.8E-04)
Received public assistance, 1979	0.42	0.41	0.42	0.42	0.44	2.19
	(3.7E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.09)
Wage and salary income, 1979	4162.45	4374.1	4217.6	4121.19	3916.55	5.09
	(41.5)	(85.67)	(80.93)	(82.05)	(82.95)	(1.6E-03)
Age of mother	29.51	29.79	29.6	29.57	29.04	14.12
	(0.04)	(0.08)	(0.08)	(0.09)	(0.09)	(3.5E-09)
Mother's years of education	11.41	11.46	11.44	11.4	11.33	1.98
	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.11)
AFDC plus Food Stamps, 1979	3.8	3.84	3.79	3.78	3.78	2.11
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.1)
AFDC plus Food Stamps, 1980	4.13	4.18	4.12	4.12	4.12	2.3
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.07)
Live in central city	0.34	0.34	0.33	0.34	0.36	2.02
	(3.6E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.11)
White	0.57	0.58	0.57	0.58	0.56	1.18
	(3.7E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.32)
No. of own children in HH aged:						
0-5	1.49	1.51	1.49	1.48	1.48	1.56
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.2)
$6-12$	0.73	0.72	0.73	0.73	0.74	0.42
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.74)
$13-17$	0.22	0.23	0.23	0.22	0.19	4.81
	(4.5E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(2.4E-03)
≥ 18	0.05	0.05	0.06	0.05	0.05	1.18
	(2.2E-03)	(4.4E-03)	(4.4E-03)	(4.1E-03)	(4.3E-03)	(0.32)
No. of other HH members aged:						
< 18	0.07	0.07	0.07	0.06	0.08	1.31
	(2.7E-03)	(0.01)	(4.9E-03)	(4.9E-03)	(0.01)	(0.27)
≥ 18	0.19	0.19	0.2	0.19	0.2	0.15
	(3.7E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.93)
N	17817	4364	4954	4364	4135	

Note: AFDC plus Food Stamps expressed in hundreds of current dollars per month. Column [6] top number is F-statistic for test of equivalence across quarters; number in parentheses is associated p-value.

Table 2: **Means and Standard Errors of Variables Used, By Quarter of Birth (Youngest child is 5)**

Variable	Full Sample	*Quarter of five-year-old's Birth* 74:II	74:III	74:IV	75:I	F
Public school enrollment of five-year-old	0.63	0.83	0.79	0.55	0.31	807.19
	(4.6E-03)	(0.01)	(0.01)	(0.01)	(0.01)	
Private school enrollment of five-year-old	0.16	0.11	0.13	0.18	0.22	46.28
	(3.5E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(1.0E-29)
Weeks of work, 1979	28.36	29.38	28.45	28.68	26.87	5.61
	(0.22)	(0.44)	(0.41)	(0.44)	(0.45)	(7.7E-04)
Employment, 1979	0.7	0.71	0.7	0.7	0.69	1.04
	(4.4E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.37)
Usual weekly hours, 1979	25.71	26.13	25.97	25.63	25.04	1.69
	(0.18)	(0.37)	(0.35)	(0.37)	(0.38)	(0.17)
Hours last week	21.5	22.03	21.88	21.6	20.37	3.63
	(0.19)	(0.4)	(0.37)	(0.39)	(0.4)	(0.01)
Current employment	0.59	0.6	0.59	0.6	0.56	3.41
	(4.7E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Received public assistance, 1979	0.34	0.33	0.34	0.35	0.36	2.13
	(4.5E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.09)
Wage and salary income, 1979	5193.45	5487.02	5304.12	5098.56	4854.7	5.6
	(56.24)	(116.83)	(108.96)	(109.47)	(114.39)	(7.9E-04)
Age of mother	30.7	31.09	30.78	30.68	30.21	10.65
	(0.05)	(0.11)	(0.1)	(0.11)	(0.11)	(5.4E-07)
Mother's years of education	11.72	11.7	11.75	11.74	11.65	0.82
	(0.02)	(0.05)	(0.05)	(0.05)	(0.05)	(0.48)
AFDC plus Food Stamps, 1979	3.51	3.57	3.49	3.49	3.48	3.74
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
AFDC plus Food Stamps, 1980	3.82	3.89	3.8	3.8	3.79	3.78
	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)
Live in central city	0.32	0.33	0.31	0.32	0.32	1.25
	(4.5E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.29)
White	0.62	0.61	0.63	0.63	0.62	0.42
	(4.6E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.74)
No. of own children in HH aged:						
0-5	1	1	1	1	1	0.0E+00
						(1)
$6-12$	0.79	0.82	0.79	0.78	0.79	0.78
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.5)
$13-17$	0.27	0.3	0.28	0.27	0.23	5.2
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(1.4E-03)
≥ 18	0.07	0.07	0.08	0.06	0.06	1.19
	(3.1E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.31)
No. of other HH members aged:						
< 18	0.07	0.08	0.07	0.06	0.07	0.94
	(3.5E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.42)
≥ 18	0.2	0.2	0.2	0.19	0.2	0.25
	(4.7E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.86)
N	10932	2646	3044	2715	2527	0.0E+00
						(1)

Note: AFDC plus Food Stamps expressed in hundreds of current dollars per month. Column [6] top number is F-statistic for test of equivalence across quarters; number in parentheses is associated p-value.

Table 3: **Means and Standard Errors of Variables Used, By Quarter of Birth (Youngest child younger than 5)**

Variable	Full Sample	*Quarter of five-year-old's Birth* 74:II	74:III	74:IV	75:I	F
Public school enrollment of five-year-old	0.65	0.86	0.81	0.58	0.32	553.57
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	
Private school enrollment of five-year-old	0.08	0.06	0.07	0.1	0.11	14.68
	(3.3E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(1.6E-09)
Weeks of work, 1979	15.95	17.42	15.11	16.06	15.27	4.58
	(0.25)	(0.51)	(0.46)	(0.51)	(0.51)	(3.3E-03)
Employment, 1979	0.49	0.52	0.48	0.48	0.47	3.39
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Usual weekly hours, 1979	17.38	18.47	16.76	17.54	16.77	2.93
	(0.24)	(0.48)	(0.44)	(0.5)	(0.49)	(0.03)
Hours last week	11.72	12.42	11.01	12.15	11.36	2.37
	(0.22)	(0.44)	(0.41)	(0.45)	(0.45)	(0.07)
Current employment	0.34	0.37	0.32	0.35	0.33	4.28
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(5.0E-03)
Received public assistance, 1979	0.55	0.54	0.55	0.55	0.56	0.56
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.64)
Wage and salary income, 1979	2525.42	2660.02	2485.99	2512	2442.22	0.75
	(54.1)	(110.32)	(106.57)	(110.21)	(104.77)	(0.52)
Age of mother	27.62	27.78	27.73	27.74	27.2	5.4
	(0.06)	(0.11)	(0.11)	(0.12)	(0.12)	(1.0E-03)
Mother's years of education	10.92	11.08	10.94	10.83	10.83	3.68
	(0.03)	(0.06)	(0.06)	(0.07)	(0.06)	(0.01)
AFDC plus Food Stamps, 1979	4.25	4.24	4.26	4.26	4.26	0.06
	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)	(0.98)
AFDC plus Food Stamps, 1980	4.63	4.62	4.63	4.63	4.63	0.02
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.99)
Live in central city	0.38	0.36	0.37	0.37	0.41	4.15
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
White	0.49	0.52	0.48	0.49	0.46	4.4
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(4.3E-03)
No. of own children in HH aged:						
0-5	2.27	2.29	2.27	2.26	2.25	1.86
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.13)
6 – 12	0.63	0.57	0.63	0.65	0.66	3.66
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
13 – 17	0.13	0.12	0.15	0.14	0.12	1.53
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.2)
≥ 18	0.03	0.03	0.03	0.03	0.03	0.08
	(2.7E-03)	(0.01)	(0.01)	(4.5E-03)	(0.01)	(0.97)
No. of other HH members aged:						
< 18	0.07	0.05	0.06	0.07	0.09	2.67
	(4.1E-03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.05)
≥ 18	0.19	0.19	0.19	0.19	0.19	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(1)
N	6885	1718	1910	1649	1608	

Note: AFDC plus Food Stamps expressed in hundreds of current dollars per month. Column [6] top number is F-statistic for test of equivalence across quarters; number in parentheses is associated p-value.

Table 4: **First Stage And Reduced Form Regressions (Youngest child aged 5)**

Variable	PS Enrollment	Weeks	Employed	Usl Hrs.	Wage/Sal	Pub Asst.	Hours	Cur. Emp.
	1979 Variables						*Last week variables*	
74:II	0.515	2.531	0.026	1.289	538.248	-0.029	1.581	0.038
	(0.012)	(0.58)	(0.012)	(0.49)	(151.893)	(0.012)	(0.521)	(0.013)
74:III	0.476	1.202	0.012	0.826	327.577	-0.01	1.17	0.017
	(0.011)	(0.56)	(0.011)	(0.473)	(146.745)	(0.012)	(0.503)	(0.012)
74:IV	0.24	1.382	0.006	0.336	104.391	-3.6E-05	0.841	0.024
	(0.012)	(0.575)	(0.012)	(0.486)	(150.57)	(0.012)	(0.516)	(0.013)
AFDC plus Food Stamps:								
1979	0.047	-2.543	-0.049	-0.417	-800.609	-0.021		
	(0.026)	(1.294)	(0.027)	(1.094)	(339.055)	(0.029)		
1980							-1.241	-0.053
							(1.027)	(0.025)
No. of own children in HH aged:								
0-5								
$6-12$	0.004	-2.719	-0.036	-2.869	-425.017	0.094	-2.737	-0.049
	(0.021)	(1.028)	(0.022)	(0.869)	(269.291)	(0.023)	(0.894)	(0.022)
$13-17$	-0.001	-0.123	0.005	-0.938	-40.31	0.068	-0.808	0.002
	(0.021)	(1.028)	(0.022)	(0.869)	(269.358)	(0.023)	(0.894)	(0.022)
≥ 18	0.029	0.355	-3.1E-04	-0.015	-60.878	-0.009	0.267	0.015
	(0.015)	(0.731)	(0.016)	(0.618)	(191.517)	(0.016)	(0.657)	(0.016)
No. of other HH members aged:								
< 18	0.01	-2.206	-0.03	-1.339	-518.452	0.037	-2.37	-0.055
	(0.012)	(0.577)	(0.013)	(0.488)	(151.251)	(0.014)	(0.519)	(0.012)
≥ 18	-0.007	1.852	0.037	1.927	279.48	-0.035	1.468	0.033
	(0.009)	(0.426)	(0.009)	(0.36)	(111.634)	(0.009)	(0.383)	(0.009)
Age of mother	-0.02	3.303	0.045	1.746	1024.376	-0.046	3.349	0.086
	(0.007)	(0.346)	(0.007)	(0.293)	(90.707)	(0.008)	(0.311)	(0.008)
Age of mother squared	2.3E-04	-0.049	-0.001	-0.029	-14.403	0.001	-0.048	-0.001
	(1.1E-04)	(0.005)	(1.1E-04)	(0.004)	(1.378)	(1.2E-04)	(0.005)	(1.2E-04)
White	-0.105	2.344	0.05	1.827	190.276	-0.082	3.297	0.07
	(0.01)	(0.475)	(0.01)	(0.401)	(124.364)	(0.01)	(0.426)	(0.011)
Mother's years of education	-0.01	1.873	0.039	1.431	562.317	-0.041	1.521	0.04
	(0.002)	(0.083)	(0.002)	(0.07)	(21.767)	(0.002)	(0.075)	(0.002)
Live in central city	-0.02	-3.639	-0.091	-3.485	-446.462	0.06	-1.917	-0.059
	(0.01)	(0.484)	(0.01)	(0.409)	(126.899)	(0.01)	(0.435)	(0.011)
N	10932	10932		10932	10932		10932	

Note: Standard errors in parentheses. All specifications include fixed effects for state of residence and five-year-old's state of birth.

Table 5: **First Stage And Reduced Form Regressions (Youngest child aged less than 5)**

Variable	PS Enrollment	Weeks	Employed	Usl Hrs.	Wage/Sal	Pub Asst.	Hours	Cur. Emp.
			1979 Variables				*Last week variables*	
74:II	0.541	0.913	0.024	0.826	-29.823	0.006	0.218	0.011
	(0.015)	(0.666)	(0.016)	(0.64)	(149.681)	(0.016)	(0.588)	(0.015)
74:III	0.489	-0.847	-0.01	-0.521	-91.971	0.004	-0.81	-0.029
	(0.014)	(0.647)	(0.016)	(0.622)	(145.446)	(0.016)	(0.572)	(0.015)
74:IV	0.26	0.081	-0.001	0.264	-65.733	-0.003	0.252	0.007
	(0.015)	(0.669)	(0.016)	(0.643)	(150.365)	(0.016)	(0.591)	(0.015)
AFDC plus Food Stamps:								
1979	0.034	0.501	0.002	1.329	-134.23	-0.036		
	(0.031)	(1.415)	(0.034)	(1.36)	(318.023)	(0.035)		
1980							0.885	0.008
							(1.12)	(0.029)
No. of own children in HH aged:								
0-5	-0.039	-5.428	-0.122	-5.398	-684.398	0.115	-5.022	-0.124
	(0.025)	(1.137)	(0.028)	(1.092)	(255.514)	(0.028)	(0.987)	(0.026)
$6-12$	0.003	-3.179	-0.064	-3.389	-498.601	0.094	-2.803	-0.067
	(0.024)	(1.078)	(0.026)	(1.036)	(242.316)	(0.027)	(0.934)	(0.024)
$13-17$	-0.015	-1.46	-0.021	-1.793	-298.252	0.058	-2.276	-0.047
	(0.025)	(1.152)	(0.029)	(1.107)	(258.95)	(0.029)	(0.998)	(0.026)
≥ 18	0.042	3.691	0.064	2.93	269.964	-0.012	3.476	0.085
	(0.026)	(1.174)	(0.033)	(1.128)	(263.813)	(0.031)	(1.037)	(0.03)
No. of other HH members aged:								
< 18	0.019	-0.334	-0.004	-0.035	280.568	0.004	0.152	-0.002
	(0.016)	(0.708)	(0.017)	(0.68)	(159.092)	(0.017)	(0.625)	(0.017)
≥ 18	0.008	2.851	0.075	3.395	421.651	-0.03	2.667	0.06
	(0.011)	(0.496)	(0.013)	(0.477)	(111.522)	(0.013)	(0.438)	(0.012)
Age of mother	0.016	2.522	0.037	1.6	528.468	-0.031	1.869	0.057
	(0.01)	(0.446)	(0.011)	(0.428)	(100.154)	(0.011)	(0.394)	(0.01)
Age of mother squared	-3.2E-04	-0.036	-0.001	-0.026	-6.845	3.5E-04	-0.026	-0.001
	(1.6E-04)	(0.007)	(1.7E-04)	(0.007)	(1.642)	(1.8E-04)	(0.006)	(1.6E-04)
White	-0.102	2.287	0.041	1.816	347.146	-0.097	2.231	0.06
	(0.012)	(0.535)	(0.013)	(0.515)	(120.347)	(0.013)	(0.473)	(0.012)
Mother's years of education	0.003	1.478	0.035	1.231	317.274	-0.036	1.241	0.035
	(0.002)	(0.098)	(0.002)	(0.094)	(22.039)	(0.003)	(0.087)	(0.002)
Live in central city	-0.025	-3.113	-0.114	-3.894	-328.874	0.081	-2.155	-0.065
	(0.012)	(0.549)	(0.014)	(0.527)	(123.326)	(0.013)	(0.485)	(0.013)
N	6885	6885		6885	6885		6885	

Note: Standard errors in parentheses. All specifications include fixed effects for state of residence and five-year-old's state of birth.

Table 6: **Estimates Of The Effect Of Access To Public School On Weeks Of Work In 1979 (Youngest child is 5)**

Right Hand Side Variable	[1] Means	[2] OLS	[3] OLS	[4] Wald	[5] QOB
PS Enrollment of five-year-old	0.632	-4.282	-1.124	4.692	3.59
	(0.005)	(0.449)	(0.426)	(1.259)	(0.987)
AFDC plus Food Stamps	3.51		-2.479		-2.679
	(0.011)		(1.295)		(1.303)
No. of own children in HH aged:					
0-5					
$6-12$	0.794		-2.738		-2.761
	(0.008)		(1.028)		(1.034)
$13-17$	0.27		-0.102		-0.14
	(0.006)		(1.029)		(1.034)
≥ 18	0.067		0.372		0.216
	(0.003)		(0.731)		(0.736)
No. of other HH members aged:					
< 18	0.069		-2.184		-2.238
	(0.003)		(0.578)		(0.581)
≥ 18	0.199		1.848		1.869
	(0.005)		(0.426)		(0.429)
Mother's years of education	11.716		1.863		1.907
	(0.025)		(0.083)		(0.084)
White	0.623		2.224		2.716
	(0.005)		(0.477)		(0.489)
Live in central city	0.32		-3.654		-3.547
	(0.004)		(0.485)		(0.488)
Age of mother	30.697		3.367		3.38
	(0.054)		(0.346)		(0.348)
Age of mother squared	974.224		-0.05		-0.05
	(3.562)		(0.005)		(0.005)
F[df]				1661.8[1]	837[3]
State fixed effects		N	Y	N	Y
Demographic controls		N	Y	N	Y
Instruments		–	–	Q1 Dummy	3 QOB Dummies

Note: Standard errors in parentheses. Sample size is 12,295.

Table 7: **Effect of Access to Public School On Labor Supply Measures (Youngest child is 5)**

Left-Hand Side Variable	[1] OLS	[2] OLS	[3] Wald	[4] QOB	[5] Predicted Means	[6] Percentage Effect	[7] Test of Overid.
Employment in 1979	-0.076	-0.013	0.039	0.04	0.675	5.946	1.492
	(0.009)	(0.008)	(0.025)	(0.02)	(0.013)	(2.952)	(0.474)
Usual hours in 1979	-3.465	-0.916	2.108	2.229	24.3	9.173	0.838
	(0.377)	(0.359)	(1.048)	(0.832)	(0.552)	(3.425)	(0.658)
Weeks of work in 1979	-4.282	-1.134	4.692	3.59	26.094	13.758	5.766
	(0.449)	(0.425)	(1.259)	(0.987)	(0.655)	(3.785)	(0.056)
Received public assistance in 1979	0.095	0.034	-0.051	-0.044	0.372	-11.928	2.949
	(0.009)	(0.009)	(0.026)	(0.021)	(0.014)	(5.561)	(0.229)
Wage and salary income in 1979	-1055.022	-426.327	1060.466	929.154	4605	20.174	2.143
	(116.207)	(111.182)	(324.693)	(258.786)	(1)	(5.583)	(0.342)
Hours last week	-3.784	-1.022	3.542	2.706	19.787	13.674	0.568
	(0.4)	(0.381)	(1.119)	(0.885)	(0.588)	(4.475)	(0.753)
Current employment	-0.085	-0.02	0.08	0.051	0.556	9.199	3.606
	(0.01)	(0.009)	(0.027)	(0.021)	(0.014)	(3.856)	(0.165)
State fixed effects	N	Y	N	Y			
Demographic controls	N	Y	N	Y			
Instruments	—	—	Q1 Dummy	3 QOB Dummies			

Note: For columns [1]–[4], the top number in each cell is the coefficient on the public school enrollment dummy in the equation estimated using the indicated row variable on the left hand side. Column [5] top number is mean of LHS variable predicted with *public* dummy set to zero and all other RHS variables at their means. Column [6] top number is ratio of Column [4] to Column [5] top number. Column [7] top number is χ^2 overidentification test statistic; associated p-value is in parentheses. Standard errors in parentheses (standard errors for [5]–[6] computed using the delta method). Sample size is 10,932.

Table 8: **Effect of Access to Public School On Labor Supply Measures (Youngest child younger than 5)**

Left-Hand Side Variable	[1] OLS	[2] OLS	[3] Wald	[4] QOB	[5] Predicted Means	[6] Percentage Effect	[7] Test of Overid.
Employment in 1979	-0.047	-0.017	0.048	0.011	0.481	2.185	4.512
	(0.013)	(0.012)	(0.033)	(0.026)	(0.018)	(5.491)	(0.105)
Usual hours in 1979	-1.978	-0.812	1.842	0.068	17.332	0.393	4.613
	(0.499)	(0.471)	(1.308)	(1.055)	(0.723)	(6.089)	(0.1)
Weeks of work in 1979	-2.47	-1.438	2.077	-0.111	16.025	-0.693	7.242
	(0.522)	(0.49)	(1.368)	(1.099)	(0.753)	(6.856)	(0.027)
Received public assistance in 1979	0.085	0.057	-0.037	0.015	0.541	2.825	0.178
	(0.013)	(0.012)	(0.033)	(0.026)	(0.018)	(4.858)	(0.915)
Wage and salary income in 1979	-500.913	-391.346	251.681	-124.619	2606	-4.781	0.271
	(113.481)	(109.792)	(297.014)	(246.405)	(1)	(9.048)	(0.873)
Hours last week	-2.259	-1.435	1.078	-0.945	12.332	-7.664	3.986
	(0.457)	(0.433)	(1.196)	(0.97)	(0.665)	(7.87)	(0.136)
Current employment	-0.064	-0.044	0.033	-0.027	0.359	-7.509	8.244
	(0.012)	(0.011)	(0.031)	(0.025)	(0.017)	(7.004)	(0.016)
State fixed effects	N	Y	N	Y			
Demographic controls	N	Y	N	Y			
Instruments	—	—	Q1 Dummy	3 QOB Dummies			

Note: For columns [1]–[4], the top number in each cell is the coefficient on the public school enrollment dummy in the equation estimated using the indicated row variable on the left hand side. Column [5] top number is mean of LHS variable predicted with *public* dummy set to zero and all other RHS variables at their means. Column [6] top number is ratio of Column [4] to Column [5] top number. Column [7] top number is χ^2 overidentification test statistic; associated p-value is in parentheses. Standard errors in parentheses (standard errors for [5]–[6] computed using the delta method). Sample size is 10,932.

Table 9: **Married Families: Effect of Access to Public School On Labor Supply Measures (Youngest child is 5)**

Left-Hand Side Variable	[1] OLS	[2] OLS	[3] Wald	[4] QOB	[5] Predicted Means	[6] Percentage Effect	[7] Test of Overid.
Weeks of work in 1979	-0.511	-0.107	0.96	1.232	21.365	5.767	1.009
	(0.19)	(0.186)	(0.476)	(0.369)	(0.222)	(1.727)	(0.604)
Usual hours in 1979	-0.161	0.218	1.032	1.407	18.527	7.595	2.502
	(0.16)	(0.154)	(0.4)	(0.306)	(0.184)	(1.65)	(0.286)
Employment in 1979	0.001	0.011	0.031	0.039	0.562	6.971	2.874
	(0.004)	(0.004)	(0.01)	(0.008)	(0.005)	(1.437)	(0.238)
Received public assistance in 1979	0.005	0.001	0.001	0.002	0.021	7.833	3.129
	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(11.558)	(0.209)
Wage and salary income in 1979	-250.588	-184.425	63.795	110.463	3536.3	3.124	1.225
	(44.882)	(43.56)	(112.41)	(86.623)	(52)	(2.446)	(0.542)
Hours last week	-0.21	0.137	1.214	1.241	15.201	8.165	2.296
	(0.158)	(0.155)	(0.397)	(0.307)	(0.185)	(2.022)	(0.317)
Current employment	0.007	0.014	0.038	0.044	0.499	8.79	1.444
	(0.004)	(0.004)	(0.011)	(0.008)	(0.005)	(1.645)	(0.486)
State fixed effects	N	Y	N	Y			
Demographic controls	N	Y	N	Y			
Instruments	—	—	Q1 Dummy	3 QOB Dummies			

Note: For columns [1]–[4], the top number in each cell is the coefficient on the public school enrollment dummy in the equation estimated using the indicated row variable on the left hand side. Column [5] top number is mean of LHS variable predicted with *public* dummy set to zero and all other RHS variables at their means. Column [6] top number is ratio of Column [4] to Column [5] top number. Column [7] top number is χ^2 overidentification test statistic; associated p-value is in parentheses. Standard errors in parentheses (standard errors for [5]–[6] computed using the delta method). Sample size is 54,868.

Table 10: **Married Families: Effect of Access to Public School On Labor Supply Measures (Youngest child younger than 5)**

Left-Hand Side Variable	[1] OLS	[2] OLS	[3] Wald	[4] QOB	[5] Predicted Means	[6] Percentage Effect	[7] Test of Overid.
Weeks of work in 1979	-0.069	0.031	0.718	1.151	13.823	8.327	2.251
	(0.173)	(0.171)	(0.435)	(0.329)	(0.197)	(2.378)	(0.325)
Usual hours in 1979	0.133	0.216	0.242	0.813	13.514	6.015	2.27
	(0.156)	(0.153)	(0.392)	(0.294)	(0.176)	(2.176)	(0.321)
Employment in 1979	0.005	0.01	0.012	0.024	0.429	5.669	3.902
	(0.004)	(0.004)	(0.011)	(0.008)	(0.005)	(1.903)	(0.142)
Received public assistance in 1979	0.009	0.005	-0.003	-5.8E-05	0.029	-0.196	0.225
	(0.001)	(0.001)	(0.004)	(0.003)	(0.002)	(9.635)	(0.894)
Wage and salary income in 1979	-137.672	-143.483	74.302	109.095	2144	5.087	1.891
	(37.982)	(37.302)	(95.385)	(71.868)	(42)	(3.331)	(0.388)
Hours last week	0.111	0.163	0.705	0.91	9.534	9.55	0.861
	(0.144)	(0.142)	(0.361)	(0.273)	(0.164)	(2.867)	(0.65)
Current employment	0.009	0.008	0.022	0.031	0.345	8.969	0.73
	(0.004)	(0.004)	(0.01)	(0.008)	(0.005)	(2.292)	(0.694)
State fixed effects	N	Y	N	Y			
Demographic controls	N	Y	N	Y			
Instruments	—	—	Q1 Dummy	3 QOB Dummies			

Note: For columns [1]–[4], the top number in each cell is the coefficient on the public school enrollment dummy in the equation estimated using the indicated row variable on the left hand side. Column [5] top number is mean of LHS variable predicted with *public* dummy set to zero and all other RHS variables at their means. Column [6] top number is ratio of Column [4] to Column [5] top number. Column [7] top number is χ^2 overidentification test statistic; associated p-value is in parentheses. Standard errors in parentheses (standard errors for [5]–[6] computed using the delta method). Sample size is 53,732.